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THE RÔLE OF CONSONANCE AND DISSONANCE IN MUSIC

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I. INTRODUCTION

This study considers the aesthetic enigma of the rapid development of the so-called 'modern' music and its apparent discrepancy with harmonic theory and scientific inquiry. Assuming that music is a compound of three fundamental variables, rhythm, melody, and harmony, it is obvious that each in itself is a changeable factor. The slightest acquaintance with musical history or even with the musical expression of different races is sufficient illustration of this fact. Yet, with the exception of one or two investigators, the conclusions from experimental acoustics, particularly in psychology, assume a more or less rigid entity, and display comparative disdain for the ethnological background of aesthetics in general, and the evolution of musical form in particular. The argument is commonly advanced that such factors are irrelevant to pure research. But is it not legitimate, scientifically as well as aesthetically, to question such conclusions as Seashore's,[1] that musical children and adults have an innate ear-mindedness which recognizes immediately and without training the 'consonance' of a major fifth—when recent research shows entire races in other geographic localities which react with equal spontaneity to the 'consonance' of a diminished third? It may be a well-demonstrated fact that a musical cross-section of a given population, subjected to standardized tests, shows the traditional diminishing consonance preferences of the octave, fifth, sixth, fourth, and so on, but it is equally demonstrable that many Upper Mongolian nomads will have nothing of such lordly intervals, and when they sing in harmonies at all, revert to a monoton-

*Accepted for publication April 11, 1927.
[1] C. E. Seashore, *The Psychology of Musical Talent*, 1919, 144.

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ous chant in a true augmented second.[2] Certain tribes of the Maya Indians in southern Yucatan 'howl' their part songs both in major fifths and major sevenths.[3] It is not the purpose here to digress into the relative aesthetics of primitive and European music, but the example does serve to anticipate the enigma of previous reference, namely, is the composer of modern music a freak of perverted tastes? Is the audience at a Stravinsky program having its psychological, or—to anticipate the Helmholtz theory—its physiological apparatus violated by the amazing sequences of seconds and sevenths perpetrated under the sacred name of Symphony? Or—to isolate the strictly scientific aspect from its practical context—what is the proper relation of consonance and dissonance in music? Is dissonance in and of itself legitimate? Or—further to limit the inqury, what is dissonance, if it is, and what is consonance, if it is? Is the psychological distinction, if there is a psychological distinction, a matter of innate 'ear-mindedness' as implied by Seashore,[4] a matter of physiology as contended by Helmholtz,[5] or is it purely an associational bias actively acquired, as described by Moore?[6]

Any psychology of music or of sound, in order to be at all functionally adequate, must provide at least conditional hypotheses in answer to these ordinary questions. Furthermore, any psychology of music, to be legitimate, must be capable of molding its experimental data with the observable facts of musical development.

Without getting entangled in the devious and dubious channels of controversy regarding the nature of music as an acoustic art or a physical science, it seems at least safe to define it as a form of behavior-response to aural stimuli which are selectively harnessed into a definite form, this form representing at any given time what is aesthetically pleasing. The probable origin of the modern connotation of 'music' is from the Greek word ἁρμονία (harmony), and is defined as a name given to the art of arranging sounds for aesthetic impression. The significance of this derivation will occur later. It takes but a glance at musical literature to ascertain that the basis of musical form, as we know it, is consonance. This concept, consonance, in turn, is ambiguous and controversial, whose proper analysis would seem the function of psychology alone. As Max Meyer has phrased it, "neither the physicist nor the physiologist can prove by physical or physiological laws why

[2] Ida Pfeiffer, *First Voyage*, 1861, 119.
[3] W. H. Prescott, *History of the Conquest of Mexico*, 1878, 92-105.
[4] C. E. Seashore, *op. cit.*, 139.
[5] H. L. von Helmholtz, *On the Sensations of Tone*, Ellis transl., 1912, 179-197.
[6] H. T. Moore, The genetic aspect of consonance and dissonance, *Psychol. Monog.*, 17, 1914, no. 73, 1-68.

we must enjoy certain combinations of tones."[7] It may be added that neither the physicist nor the physiologist can explain the historical shift in concord, nor certain characteristic racial prejudices, nor the affective appeal of a Ravel or a Schoenberg composition to some people. It may likewise be added that no psychologist has adequately performed that service either.

The present study is therefore an attempt to isolate certain aspects of the single musical factor of consonance and to correlate them, if possible, with the dissonance tendencies in modern musical composition.

In spite of the fact that harmony texts are founded on 'consonance,' that musical form has been harnessed in it, that psychological, physiological, and physical treatises assume it, there is nowhere available a uniform conception of the term. Thus is occasioned a preliminary obstacle to experimental approach and experimental technique. For if the ideas of an entity itself are contradictory, the conceptions if its attributes, in fact the very criteria which gauge its existence are perforce contradictory and ambiguous. The following summary of definitions, sufficiently augmented by criticism to make their import intelligible, serves well as a measure of this quandary. In general, two contentions may be *a priori* granted; first, no one denies the relationship apparent between the simplicity of numerical ratio and the simplicity of sensation in a two-tone clang; second, with the exception of the early Greeks, pleasantness and unpleasantness as the sole criteria of consonance, have been definitely outlawed. However, it is highly significant to notice the frequency with which the latter idea occurs in modified garb as circumstantial evidence for consonance.

Descartes, Leibnitz, Euler, and Schopenhauer are ordinarily classified together as the chief exponents of the purely Pythagorean mathematical basis of harmony. Leibnitz amplifies the theory with a convenient hypothesis of "unconscious calculation,"[8] and Euler includes the affective element in consonance as a function of the ease of perception.[9] Schopenhauer's analysis is a metaphysical transcription of arithmetic.[10] These theories, as has been frequently pointed out, state a factual phenomenon, but offer absolutely no explanation of it whatever. A most obvious refutation lies in the simple experiment of slightly mistuning these perfect intervals, thereby rendering their mathematical ratios exceedingly complex, but effecting no appreciable change in their consonant value.

The four outstanding historical figures in divergent theory, as well as in experimental mass, are Helmholtz, Lipps, Krueger, and Stumpf.

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[7]Max Meyer, Contributions to a psychological theory of music, *Univ. Mo. Studies*, 1, 1901, 1.

[8]G. W. Leibnitz, Principes de la nature et de la grace, *Leibnitii Epistolae ad Diversos*, Epistola 154.

[9]L. Euler, *Tentamen Novae Theoriae musicae exercitissimis Harmoniae Principiis dilucide expositae auctore*, 1739, Cap. II, III.

[10]A. Schopenhauer, The world as will and idea, *The Metaphysics of Music*, iii, 235.

The Helmoltz theory is an evolutionary one of three stages,[11] developing from an early associatory basis into a detailed analysis of overtone and difference tone relationships. Its context is too well known to demand other elaboration than the familiar summary, "consonance is a continuous, dissonance an intermittent sensation of tone,"[12] the latter comparable in sensory effect, according to his opinion, to the visual unpleasantness of flicker. Dissonant intervals always involve the roughness of beats. These beats may accrue from the primaries, as in the minor second, or from their upper partials, as in the case of the fourth partial, e of c′ beating with the fourth partial, f of f′; or from a partial of one tone with the other fundamental, as in the third partial, b of e′ beating with the other primary in the major third, C′-e′. The third aspect of the Helmholtz theory concerns the effects of difference and summation tones, not only those between the fundamentals, but also those resulting from harmonic combination tones. Without digressing too far into the intricacies of his aural theory, the possibility of perceptible beats between the difference tones of the higher partials is somewhat difficult to conjecture. Assuming that the difference tone is generated internally in the first place with its only resonator comprising the appendages of the middle and outer ear, it is pertinent to ask exactly how the difference tones of the high partials are generated, whether they meet the same resonator device, and above all, whether their intensity is actually sufficient for perceptual effect. There is voluminous literature at variance with the beat theory of dissonance, but the only two universal objections are as follows:

(1) That beats may occur without dissonance and vice versa. For instance, a pure tone clang of 600 and 900 vibrations produces 300 beats between its primaries, which are well above the limen of beat perceptibility. Theoretically, therefore, this clang should be consonant, yet it produces the effect of a very marked dissonance.

(2) Monaural perception of dissonant primaries, or the condition of diplacusis will eliminate beat interference but will not effect a consonance metamorphosis.[13] In passing, it is pertinent to add that Peterson[14] regards this criticism of Helmholtz as invalid, in that these beatless discords may be due to habituation.

Regardless of the controversy, the Helmholtz definition is apparent. He attributes to consonance a physiological, *i.e.* a sensorial basis, with smoothness (identity of overtones and lack of beats among partials and combination tones) as its foundation. (Apropos of affective inference, we find this statement repeatedly occurring, "The cause of the *unpleasantness* of dissonance is attributable to this roughness and entanglement.")[15]

Krueger,[16] though apparently disdaining the detailed accounts of his predecessor, really has adopted and modified the third phase of the Helmholtz theory, by shifting the major responsibility for dissonance from the overtones to the difference tones. Thus, even if two primaries are of sufficient distance to prevent direct interference, the difference tones generate beats, with their attendant blurring and roughness. For instance, there are at least three difference tones of 100 vd. each, generated by the interval, 200-300 vd. These three difference tones, in reality, are identified with each other, or with what Krueger calls the "characteristic" difference tone, and the interval is thereby consonant. By slightly mistuning the interval to 200-307 vd. the difference tones change to 107, 93, 14, 79, and

[11]Helmholtz, *op. cit.*, 225.
[12]*Ibid.*, 226.
[13]The author is at present investigating the possibility of bone conduction of beats with monaural primaries of high intensity. It is possible that this wide-spread assumption may need modification.
[14]J. Peterson, A functional view of consonance, *Psychol. Rev.*, 3, 1925, 17-33.
[15]Helmholtz, *op. cit.*, 226.
[16]Felix Krueger, Die Theorie der Konsonanz, *Psychol. Studien*, 1, 1906, 305-387.

so forth. These tones lie in close proximity, and hence generate beats, effect roughness, and consequently abolish the consonant nature of the tuned clang. This theory obviously does not account for the dissonance of such intervals as 11:8, with its difference tones 3, 5, 2, 1, 1. As Stumpf points out, a fifth is in this instance the minimal interval, and hence should produce no beats. The second criticism of this theory is that it, too, fails to explain the beatless dissonance of diplacusis.

Theodore Lipps[17] augments the Euler hypothesis of simple ratios and simple physical rhythms with the metaphysical assumption of an "unconscious rhythm." For example, an interval of 300-200 vd. stimulates two correspondingly separate unconscious experiences, but these latter may combine into one conscious experience. If the ratio be simple, as 2:1, the fused conscious effect will have the 'pleasant' nature of a consonance; or, in neurological terms, if two frequencies have the ratio 2:1, every second nervous excitation coincides, but with a ratio of 8:9, there is non-rhythmic excitation. This rhythm is, per se, unconscious, but Lipps would endow it with the same psychological function as a perfectly objective dance-rhythm. The obvious criticism of such theory is the same one applicable to the Freudian conception of art in general, to wit, the assumption of an "unconscious" process with a consciously perceiving function, regardless of whether such process is called a "micro-psychic" or a "libido."

Next to Helmholtz, Stumpf[18] has contributed more definite experimental data toward the problem then any other single investigator. Taking a new departure, he restricts the meaning of consonance to *fusion*, defining fusion in turn as "that relation of two sensation-contacts in which they form a sum but not a whole."[19] In his opinion the criteria of consonance must not only be conscious entities, but objective tonal attributes as well. Though he himself denies it, the very technique of his experiments synonymises fusion with the unanalyzability of the sensation. Low degrees of fusion mean high degrees of analysis, and vice versa. The objective factors in harmonic experience, commonly known as the Stumpf laws, are an important technical contribution, and will be later referred to in connection with the problems of experimental control. It may be said in passing, however, that as a study in tonal fusion, his experiments suffice admirably; as an explanation of consonance, they fail. They reassert a sharp line of demarcation between consonance and dissonance, and attribute to each an elemental and inviolable simplicity of sensory nature. They can in no way account for the harmonic shifts of the last century, nor for the anomalous aesthetic principles of post-Wagnerian music.

In addition to Wundt, for whom consonance is merely a tonal synthesis effected by the 'apperceptive' faculty of the mind, these four investigators, though widely divergent in view, comprise what we may call, by analogy to contemporary religion, the 'consonance-fundamentalists.' Each has his disciples and dissenters, Tartini[20] and Pryor[21] following chiefly the Helmholtz tenets; Faist,[22] Meinong and Witasek, [23] and Pear[24] differing only in degree from the original Stumpf. Authors of psychological texts and experimental manuals tend similarly to classify their discussion material after one or the other of these principal schools. Titchener[25] uses the terms

[17]Theodore Lipps, Das Wesen der musikalischen Harmonie und Disharmonie, *Psychologische Studien*, Heidelberg, 1885.

[18]Karl Stumpf, Konsonanz und Dissonanz, *Beiträge z. Ak. u. Musikwiss.*, 1, 1898, 91-107.

[19]Stumpf, *Tonpsychologie*, ii, 1883, 127 ff.

[20]Tartini, *Tratto di Musica*, 1754.

[21]Wilhelm Preyer, Zur Theorie der Konsonanz, *Akustische Untersuchungen*, 1899, 44.

[22]A. Faist, Versuche über Tonverschmelzung, *Zsch. f. Psych.*, 15, 1897, 189-205.

[23]A. Meinong and S. Witasek, Zur experimentellen Bestimmung der Tonverschmelzungsgrad, *Zsch. f. Psychol.*, 15, 1897, 189-205.

[24]T. H. Pear, Differences between major and minor chords, *Brit. J. Psychol.*, 4, 1911, 56ff.

[25]E. B. Titchener, *Experimental Psychology: Qualitative, Instructor's Manual*, 1907, 329.

consonance and fusion more or less synonymously; Sanford[26] accepts smoothness and the absence of beats as the basic criteria. Pillsbury[27] states that consonance depends partly on the degree of fusion, but more upon the training of the individual. Both he and Ogden[28] would add to the purely cognitive and physical combinations the factor of feeling tone. The physicist Barton[29] emphatically substantiates Helmholtz, reserving however, that the degree of dissonance or roughness is dependent not wholly on the interval, but to a marked extent on the clang or quality of each of the component tones.

Malmberg,[30] on whose experimental data Seashore directly bases his own conclusions, practically if not theoretically, allies himself with the 'fundamentalist' school. His criteria for consonance are four-fold—blending, smoothness, fusion, and purity; for dissonance, they are disagreement, roughness, disparateness, and richness. His definition is as follows: "When the two tones of a two-clang tend to blend or fuse and produce a relatively smooth and pure resultant, they are said to be consonant. Dissonance is the reciprocal of this."[31] Both Malmberg and Seashore conceive the nature of consonance as an elemental sensory capacity differing innately in individuals and have proceeded on this basis to standardize and commercialize tests for determining its presence and degree. To quote Seashore directly, "The ability to judge the quality of two-clang as in consonance is now the most general test of sensory capacity for musical intellect."[32] And again he refers to it as a "natural sense" independent of the vagaries of feeling, and dependent primarily upon pitch discrimination. For practical purposes, in detecting "ear-mindedness" (*for diatonic intervals!*) this theory may suffice. But in its turn, it is too rigid to explain either the actual historical and aesthetic aspects of consonance, or the chromatic tendencies in the 'new music.' Malmberg emphatically states that the perception of consonance is a "purely sensory and cognitive" function; that affection, being of high variability, is definitely subsidiary to all other criteria in the aesthetic judgment as a whole. Seashore however, comprises with the statement that consonance is *ultimately* feeling and that we must not minimize its affective rôle in the 'enjoyment' of harmony and the 'jarring' of dissonance. It is somewhat difficult to reconcile these varying ideas.

Though evading a definite personal opinion, Külpe's ideas are of interest because he considers consonance in its practical relation to music. For him consonance and dissonance, and their musical counterparts, harmony and disharmony, are perceptual aspects of simple fusion. Fusion, in turn, is a function primarily of the relative *intensity* of the clang primaries in facilitating or in hindering the analysis of the tonal perception. 'It cannot be denied," Külpe says, "that the principle affords a real theory of musical harmonics and a theory which covers a large proportion of the facts. Yet in certain respects it remains inadequate."[33]

As opposed to these pure and hybrid 'fundamentalists' we have on the other hand, a group of pure and hybrid 'associationists.'

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[26]E. C. Sanford, *Experimental Psychology*, 1908, 78 f.

[27]W. B. Pillsbury, *Fundamentals of Psychology*, 1922, 147 f.

[28]R. M. Ogden, A contribution to the theory of tonal consonance, *Psychol. Bull.*, 6, 1909, 297-303.

[29]E. H. Barton, *Textbook of Sound*, 1922, 472-484.

[30]C. F. Malmberg, Perception of consonance and dissonance, *Psychol. Monog.*, 25, 1918, 93-133.

[31]Malmberg, *op. cit.*, 108.

[32]C. E. Seashore and G. H. Mount, Correlation of factors in musical talent and training, *Psychol. Monog.*, 25, 1918, 83.

[33]Oswald Külpe, *Outlines of Psychology*, tr. 1901, 306.

Max Meyer definitely differentiates harmony from consonance.[34] He insists that harmony is as much a function of successive as it is of simultaneous tones, but that consonance is a unique by-product of the latter. He offers explanation no further than to say, "All we know is that the degree of consonance depends in some manner upon the simplicity of numerical relations, similarly as melodic relationship does. In duads, we observe a high degree of consonance where we find a close relationship, a low degree of consonance where we find a remote relationship or no relationship at all. In triads, tetrads, and more complex chords there is no such simple relation between the consonance and the relationship."[35] Meyer would outlaw the term "dissonance" as having no scientific value, and speak merely of lesser degrees of consonance. Later experimental studies, indicating personal habituation as a highly pertinent factor in judgment of consonant intervals, lead him eventually to conclude that it is purely a psychological matter as to why we enjoy certain combinations of tones.[36]

Somewhat in advance of his time, Watt assumes the simultaneous vibration of the entire basilar membrane and develops accordingly a volume-theory of consonance, which he seems to define as fusion. It is physically, if not psychologically demonstrable, according to Watt, that "the volumes of all tones that have the ordinary fusional relationship with each other, are inversely proportional to the corresponding rates of physical vibration."[37] Fusion means the whole mass of sound formed by the two tones. Contemporary research seems to indicate that the entire basilar membrane, does, as a matter of fact, vibrate simultaneously, but it is difficult to see why this fact would legalize a hypothesis of balanced and unbalanced tonal volume. The association element in the Watt theory, while not original with him, is less disputable. It merely reiterates the generally known fact that in tones which we hear, including the sounds of nature and the human voice, those partials which occur loudest and oftenest are in order the octave, the fifth, the second octave, the third, and so on. It is a psychological truism that what we hear often together tends to become, if not definitely pleasing, at least anticipated.

Washburn[38] amplifies this habituation factor in a somewhat anthropomorphic fashion by saying that a two- or three-tone combination which emphasizes other than these habitually audible partials, produces confusion in an ear so naturally and constantly sensitized to the octave and fifth. The mental concomitant is one of unexpectedness, surprise, and novelty, and consequently, unpleasantness. The demand for a return to the accustomed would account for the existence of a musical 'tonic' or 'key-note.' Indisputable as these natural overtone intervals are, and logical as her derivation seems, there is, after all, a great quantity of music, both ultra-modern and aboriginal, which has no tonic to return to.

Emerson, working with reduced intervals, has demonstrated, at least to his own satisfaction, that "The whole apparently natural demand for the tone-combinations which give fusion (or consonance) can be inhibited during the listening to a-musical combinations as soon as a short training in miniature intervals changes the acoustical perspective."[39] The fact that our tone-consciousness has been trained in our musical tone-relations under the guidance of harmonic experiences is responsible for our apparently instinctive aesthetic preferences. This analysis would obviously reduce consonance, or at least its affective constituents, to pure association.

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[34] Max Meyer, *op. cit.*, 60.
[35] *Ibidem.*
[36] *Idem, The Musician's Arithmetic*, 1925, 1-25.
[37] H. J. Watt, *The Foundations of Music*, 1919, 20.
[38] M. F. Washburn, Psychology of aesthetic experience in music, *N. E. A. Pub.*, 1916, 600 f.
[39] L. E. Emerson, The feeling value of unmusical intervals, *Harvard Psychol. Studies*, 1906, 269 f.

Wood[40] makes our present classification of consonant intervals the by-product of diatonic evolution, and the diatonic system in turn is a by-product of the natural differences between the male and female voice, which led to the repetition of ceremonial melodies an octave or a fifth apart. His evidence rests historically on the late appearance and very slow growth of the harmonic system in music. As will be later indicated, Wallaschek, the foremost authority on primitive music, tends to refute the belated appearance of harmony.[41] There is also dispute as to which is cause and which is effect—our diatonic system, or its 'consonant' intervals.

Buch,[42] experimenting with both fusion and smoothness as criteria, decides that "fusion" is an unnecessary term as applied to consonant relationship, and somewhat drastically reduces the entire phenomenon to the ordinary laws of association.

Peterson,[43] one of the more recent experimenters, propounds what he calls a "functional view" of consonance. In reality, it is the Helmholtz-Krueger theory plus the social-genetic theory of Moore. According to Peterson, consonance is too complex to be called an "elemental capacity" in the Seashore sense, but it is rather a matter of synthetic perception comparable to the perception of an overtone clang, thoroughly conditioned in turn by socially transmitted musical habit.

Ogden[44] asserts a somewhat similar theory, except that his genesis of concord is biological, not social. Consonance is for him a perceptual synthesis, which by virtue of centuries of repetition of the 'natural' overtone intervals has made these discrete elements a consonant 'unity.' "The nervous correlate of consonant relationship," he says, "I regard as nothing other than a relatively simple and economic activity on the part of the sense organ and nervous system. Such dispositions, I take it, are in the nature of fundamental capacities resulting from very frequent racial experiences." The chief objection to this theory is the assumed transmission of acquired characteristics—which, with the current change in biological attitude, may be eventually eliminated.

There remains to discuss briefly the comprehensive genetic survey of Moore.[45] Tonal fusion, for him, means simply a highly permanent connection wrought under the influence of repeated sensory coincidences, "the nature of whose ancestry has been too little considered."[46] According to this conception, the first and original dissonance was the octave, prior to which all music was in unison. This dissonant interval, having crossed somehow over the borderline of usage, occasioned perceptual conflict, hence an emotional reaction of pleasure-pain, with its successful incorporation into the musical system finally leading to a dominance of pleasure. As the ear became accustomed to it, its affective value correspondingly suffered diminution, and since there was no struggle involved, it became indifferent. Musical innovators then inaugurated the fifth, which followed a similar career. The fourth, the sixth, and the third came in due turn, though relatively late historically, and now the diminished seventh is over the threshold. He says, to quote him formally: "The inherent feeling value of a particular interval is a function of two factors, success and degree of difficulty. It is in the barely successful synthesis (fusion) in which the individual accomplishes with difficulty the unification of a manifold, that he finds the keenest pleasure. If the synthesis is effected without

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[40] James Wood and C. K. Ogden, *Foundations of Aesthetics*, 1922.

[41] Richard Wallaschek, *Primitive Music*, 1893, 139-144.

[42] Einar Buch, Über die Verschmelzungen von Empfindungen besonders bei Klangeindrucken, *Phil. Stud.*, 15, 1900, 240.

[43] Peterson, *op. cit.*, 20-33.

[44] R. M. Ogden, *op. cit.*, *Psychol. Bull.*, 6, 1909, 297-303.

[45] Moore, *op. cit.*

[46] *Ibid.*, 4.

effort, he has little awareness of his accomplishment, and therefore little pleasure. If, on the other hand, he is baffled by the complexity of the manifold to be unified, he experiences acute displeasure."[47]

All perfect consonances, according to this theory, would be indifferent —as they actually are; all imperfect consonances would be pleasant; and all dissonance would be acutely unpleasant. So far, the theory seems to account for the vagaries in aesthetic reaction to tonal combinations, and particularly for their historical shift. But Moore is an extremist, and summarizes thus: "We must expect that our whole system of harmony will gradually come to reflect whatever changes occur in the consonance degrees of its constituent intervals; and this latter type of change is one to which no limit can be set."[48]

If this infinity of limit were correct, we should expect music eventually to become an obsolete art. For, having accustomed ourselves to all its available intervals in all its conceivable scales, we should sometime strike the physical limits of audition, and if there were no more 'dissonances' to incite our pugnacious instinct, and all the other intervals had through habituation approached the stage of indifference which the octave and fifth now presumably hold, there could be no further emotional or aesthetic appeal in harmony. Perhaps this criticism is far-fetched. Certainly Moore provides us with a good description of what happens in musical adaptation to new concords, but as will be pointed out later, he has fundamentally erred in extending his boundaries beyond the natural physiological activities of the aural mechanism.

To deviate momentarily from scientific paths and venture over the boundary of aesthetic theory, we find the same inconsistency of opinion. Gurney[49] withdraws himself from the complicated relation of consonance and music, just as he begins to suggest acceptance of the Helmholtz theory. Parker[50] states that consonance is the basis of musical harmony, though not identical with it, and attributes to dissonance the aesthetic function of contrast. His criterion is smoothness, and his explanation is essentially the Helmholtz principles. In comparison, Croce[51] is not such a good psychologist, and is content to imply that music as an art transcends pure sensory and physical laws.

The conclusion to be derived from the résumé of the literature is that *consonance* is not *accordance*—not at least by definition. Yet, historically and experimentally, the controversialists classify themselves into definite categories:

(1) Those who make sensory fusion a criterion for consonance.

(2) Those who make perceptual 'smoothness' the criterion.

(3) Those who make feeling and affection the criterion.

(4) Those who make habit or familiarity the criterion.

Correspondingly, their explanations classify themselves as follows:

(1) Consonance as due to the numerical relationship of the primary tones.

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[47] *Ibid.*, 38.
[48] *Ibid.*, 68.
[49] Edmund Gurney, *The Power of Sound*, ii, 1880.
[50] DeWitt Parker, *The Principles of Aesthetics*, 1920, 161-165.
[51] Benedetto Croce, *Aesthetic*, Tr., 1922, 104-118.

(2) Consonance as due to the absence of beats among the overtones and difference tones.

(3) Consonance as due to the innate feeling value of certain intervals.

(4) Consonance as due (a) to ears naturally sensitized by frequent stimulation of certain partials; and (b) to the social transmission of an arbitrary harmonic system, of which we are the habit victims.

II. Problem

The specific problems attacked in this study are: (1) The comparison of the judgments of all the musical intervals of the tempered scale according to (a) fusion, (b) smoothness, and (c) affective value, *i.e.* pleasantness or unpleasantness. (2) The comparison of the judgments given by (a) musically untrained subjects, (b) subjects with a moderate amount of musical training, and (c) very advanced musical performers—principally concert or teaching musicians. (3) The construction of apparatuses (a) to provide a stimulus series of pure tones of two different intensities, and (b) to provide a duplicate series of musical clangs with pronounced overtones.

Apparatus and Method. With modern invention and electrical equipment facilitating the generation of pure tone and the control of its intensity, the physical bases of music will undoubtedly enjoy much more accurate analysis than they have in the past. Historically, apparatus for determining consonance and dissonance has been limited to the clangs of various musical instruments, or to the pure tones of the tuning fork. Helmholtz worked principally with violin tones, though he supplemented them by a number of other generators; Stumpf and Buch with pipe organ tones of different stops; Krueger and the more recent investigators with tuning forks and Tonmesser. Malmberg used piano, tuning fork, and pipe organ tones.

Theoretically, and especially if the overtone hypothesis holds true, both the clang type and the specific condition of any musical generator ought to yield results not strictly equatable nor accurately comparable to those of a different medium. Thus, by virtue of its prominent third, fourth, and fifth partials, an interval played on the violin could not produce the same *physical* fusion as the interval played on the clarinet, whose construction permits its seventh, eighth, ninth, and tenth partials to average each from 15 to 18% of the total loudness; and neither ought theoretically to produce the same effect as the interval generated by the simple tones of forks and resonators. Collective experiments do demonstrate a discrepancy in data, but since the net result seems anyway to fall into the conventional harmonic classification, very little attention has been paid to this variable factor.

Method and technique show the same variations as do apparatus and theory, ranging from Euler's purely mathematical calculation and the direct analyses of Stumpf and his disciples to the current method of making paired comparison judgments under each separate criterion. Külpe's suggestion for elaborating the reaction-time experiment is an intriguing one, but with the exception of Max Meyer's work, very little has been done with its possibilities.

For this experiment, three types of generator were selected: mounted Koenig forks to provide low and medium intensities of pure tone; Helmholtz resonators to provide high intensity of pure tone; and organ pipes to provide the overtone-clangs. The method of actuating forks is really a serious problem in technique. Even if electrically driven, so that intensity is controlled, there remains the doubtful period preceding maximum vibration. Since our results from tuning forks serve principally as a check against the Helmholtz resonators, we have contented ourselves with a simple adaptation of pendular striking. Two hard rubber balls, mounted on metal rods and suspended in a wooden frame at an appropriate angle are raised at every trial to a level of 45° and allowed to fall 2 sec. after the warning signal. In this way, at least some effort is made to equalize the energy of stimulation between the sounding primaries.

A familiar variation of this method involves the actuating of several tuning forks with the subsequent damping of all but the fundamentals of the desired interval. A comparison of data from the two methods will show discrepancies and very interesting introspective reactions apropos of auditory imagery.

The second apparatus consists of a set of mounted Helmholtz resonators, equivalent in pitch to the tuning forks.[52] The resonators are individually actuated by a set of valves controlling the admission of compressed air, the force being equalized by means of a subsidiary constant pressure air tank.

In the third part of the experiment, we use a series of open organ pipes, similarly actuated with uniform pressure by attaching them to the rubber conductors in place of the resonators. The pipes are selected from the 'wood-wind' series and reproduce quite audibly the first, third, and fifth overtones in addition to the fundamental.

The intervals common to all series are as follows: unison, major and minor second, major and minor third, perfect fourth, perfect and diminished fifth, major and minor sixth, major and minor seventh, and octave. The tonal range extends from c' of 256 vd. to c'' of 512 vd.

In accordance with Stumpf, any investigation of consonance must take into consideration the following objective variables: distance on the tonal scale, absolute and relative intensity, spatial separation, the number of fusing tones, the duration of the clang, partial tone exchange, the subjective variables of attention, practice, fatigue, and memory. Külpe would add clang analysis of the connecting tones, combination tones, expectation, and habituation. We should add, as a very important though neglected factor, *sequence*. Obviously any single one of these many factors is sufficient in itself for a problem in research. But it is equally obvious that in isolating any one intensively, the others must be gauged as accurately as possible for correct technique. Accordingly, attempt has been made in this experiment to control intensity, duration of the stimulus, sequence of interval, and the various psychological elements of attention, fatigue, habituation, and the musical background of the individual observers.

Preliminary to his experimental observations, every *S* filled out a questionary, which, for purposes of standardization, follows the one in Seashore's text. In addition, these directions were appended: (1) Name, in order of preference, your favorite musical composers. (2) Describe briefly the elements of musical theory you have studied.

This brief questionary makes no pretense of covering the detail in the individual's musical nature and musical history. It is merely an index to

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[52]This apparatus was first set up for elementary laboratory experiments by Carl R. Brown, instructor in psychology. The principal modification here is the addition of easily regulated valves to govern the air supply.

his degree of training, the type or types of music in which he is interested, and the possible harmonic prejudices he may have acquired in his experience.

Subjects. We used 106 *Ss* in the experiments; 56 of the *Ss*, students in experimental psychology, were musically untrained; 34 of them, members of musical sororities, were moderately trained; and 16, members of the staff of the University of Michigan School of Music and other professional musicians, were highly trained.

For experimental work *S* was seated with his back toward the apparatus, with the sound source 8 ft. away approximately in a straight line with the median plane of his head. All stimuli were binaural. Warning signals of 2 sec. preceded the stimulus, and rest intervals of 10 sec. were interpolated between every two series to counteract fatigue. *S* used prepared blanks on which to record his reactions. Whereas Malmberg used the method of knowledge and permitted his *Ss* to discuss their judgments before recording them, we preferred to employ strictly the method of ignorance wherever possible.

Experimental series. The complete experimental series were as follows.

Series Ia. Fusion was taken in Series Ia as the criterion of judgment. Duration of the clang was 3 sec. Time interval between stimuli was 6 sec. The method was that of direct analysis, *i.e. S* recorded his judgment regarding the number of tones heard. If a judgment was easily made *S* marked it with a plus sign, if made with difficulty he marked it with a minus sign. For statistical reasons no indifferent judgments were permitted, they all had to be marked as easy or difficult. The clangs were given in haphazard order, but so distributed that every interval in the scale followed at some time during the experiments every other one.

Series Ib. Backward repetition of Series Ia. Duration of the clang was increased to 10 sec., and the time interval between stimuli to 20 sec. Series Ib was in other respects the same as Series Ia.

Series II. Tuning forks: procedure similar to that of Series Ia and Ib.

Series III. Organ pipes; procedure similar to that of Series Ia and Ib.

Series IV. Tuning forks; group actuated, damped selectively; otherwise procedure similar to that of Series Ia and Ib.

Series V, VI, and VII. Repetition of Series I, II, and III with smoothness as the criterion of judgment. No arbitrary *Aufgabe* was imposed, the *Ss* were permitted to interpret 'smoothness' in their own manner, just as they do under ordinary circumstances when they listen to music.

Series VIII, IX, and X. Repetition of Series I, II, and III with the affective value of the stimuli (pleasantness, unpleasantness, indifference) used as the criterion of judgment.

Series XI, XII, and XIII. Tuning forks, resonators, and organ pipes were used respectively to provide the single tone stimuli. Otherwise procedure similar to that of Series Ia.

TABLE I

SUMMARY OF RESULTS

Group of *Ss*	No. *Ss* in Group	No. of Trials per Interval	Total No. Trials per Interval	No. of Intervals	Total No. Trials per Group
I. Untrained	56	10	560	12	6720
II. Moderately Trained	34	10	340	12	4080
III. Highly Trained	16	10	160	12	1920

Results. For the most part the quantitative results of the experiment are translated into curves which are given in Figs. 1-14. Unless otherwise indicated, the ordinates represent either the percentage of error, or the percentage of absolute judgments, and the abscissae represent the musical intervals employed. Since the curves give the data of the tables it was deemed unnecessary to include the latter. With the exception of Table I, which gives the summary of the results, we have therefore omitted the tables.

From analysis of the curves in the accompanying Figures, it will be seen that the results for smoothness and for fusion are approximately the same. The sequence with the resonators for Group I is as follows:

GROUP I (Resonators)

Fusion:	8	5	M6	4	M3	m3	m6	m7	m5	M7	M2	m2
Smoothness:	8	4	5	M3	M6	m3	m7	m6	m5	M7	M2	m2

The differences between the curves concern chiefly the position of the perfect fourth, and the sequence of the thirds and sixths, the first two and the last two intervals of the series being strikingly coincident. Reference to the curves for tuning forks and pipes will show a similar consistency of result, the variations being small at both ends, and occurring principally in the region of the major and minor thirds and sixths. Practically the same relationship holds within the data for Group II and Group III.

GROUP II (Resonators)

Fusion:	8	4	5	M6	m3	m6	M3	m7	m5	M2	M7	m2
Smoothness:	8	5	M3	M6	4	m7	m3	m6	m5	M7	M2	m2

GROUP III (Resonators)

Fusion:	4	5	8	M3	M6	m3	m6	m5	m7	M7	M2	m2
Smoothness:	8	5	4	M6	M3	m6	m3	m7	m5	M2	M7	m2

There is a noticeable variation in amplitude which characterizes the curves for different stimuli among Group III, which at first seems inconsistent. It is perhaps best explained by the following excerpts from introspective reports:

R. L. (concert pianist): It is hard to regard each separate stimulus as such. I usually recognize each tone of the combination, even with the octave. Then I have to stop and think, 'What is its *smoothness* value?' I find myself invariably comparing it either with the preceding stimulus, or with a mental image of various other musical intervals. Now a major sixth, for instance, from the organ pipes, seems to me to blend into a smoother sound than a major sixth with the tuning forks, or perhaps I am just thinking that the overtones ought to cover up the separate primary tones. In other words, I find myself rationalizing, and comparing and classifying, instead of regarding each chord as a separate sensation experience. I doubt whether a trained musician *could* really do the latter.

S. W. (professional organist): I find it hard to overlook musical theory and harmonic experience in my judgments. That is, it is hard to say whether a major seventh is "smooth" without having an actual context or supplying it with an imaginary one.

This tendency to rationalization is fairly prevalent among the *S*s of Group III. The reactions of the members in Group I are, on the contrary, fairly devoid of such empirical ramifications, and their experiences more closely approach a strictly sensory aspect. They show little inclination to dissect each

FIG. 1. FUSION DATA FOR GROUP I
The ordinates represent the percentage of error (judgments of 2 tones as one tone). The abscissae represent the musical intervals

clang, and are less guilty of confusing the affective with the sensory element. Ignorance works exceedingly well in the case of their judgements of fusion, and they show little hesitancy in deciding which clangs are 'smooth' and which are 'rough,' in comparison with the other group.

It is interesting to note in passing the striking differences between the fusion curves of Malmberg and those of this study. We compare below Malmberg's results with the weighted results from Group I.

Malmberg:	8	m2	M2	M5	m3	M3	4	m5	m6	M6	m7	M7
Group I:	8	5	4	M6	M3	m3	m6	m7	m5	M2	M7	m2

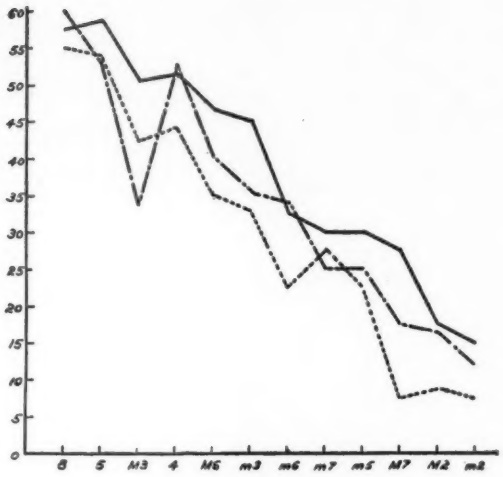

FIG. 2. FUSION DATA FOR GROUP II
The ordinates represent the percentage of error; the abscissae represent the musical intervals

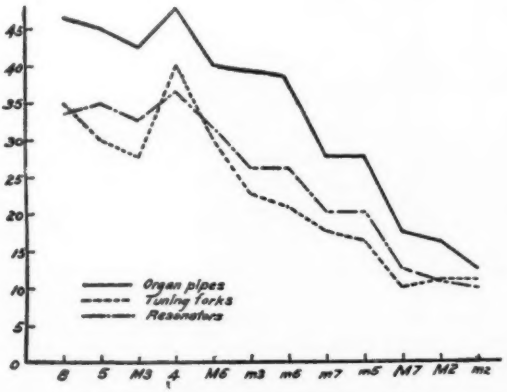

FIG. 3. FUSION DATA FOR GROUP III
The ordinates represent the percentage of error; the abscissae represent the musical intervals

The most feasible explanation of these discrepancies is the difference in the technique employed. Our *Ss* were unaware of the actual number of tones sounded at any time. They were furthermore given the simple direction, "listen carefully to each one of the following series of tonal stimuli which will be given after a warning signal, and record the number of tones heard in each case. If it is difficult to make a judgment, record also a minus sign; is it easy, record a plus sign."

Malmberg,[53] moreover, defined 'fusion' for his *Ss*, and his experiment was, as he says, "made 'with knowledge,' and was conducted on the plan of an informal seminar, allowing a discussion of each judgment, but each observer recording his own final judgment."

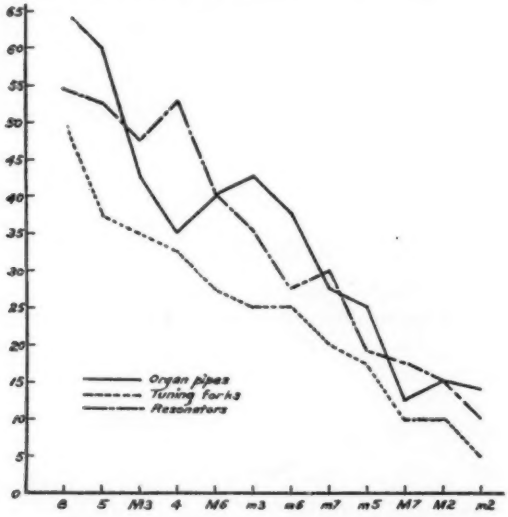

FIG. 4. SMOOTHNESS DATA FOR GROUP I

The ordinates represent the percentage of total trials in which an interval is called "smooth." The abscissae represent the musical intervals

The fusion curves obtained in our study agree, on the other hand, rather closely with the curves obtained by Stumpf, who gives the following sequence:

Stumpf:	8	5	4	M3	M6	M2	M7
				m3	m6	m2	m7

The influence of tone-quality appears in the amplitude rather than in the absolute form of the curves for different generators. The results show with marked consistency that organ tones fuse the most easily, with resonators second, and tuning forks last in effect. This difference cannot be due

[53] *Op. cit.*, 109 f.

merely to differences in intensity, since the former two are actuated by the same absolute energy, mechanically controlled. The only plausible explanation seems therefore, to be in terms of overtones. The ear is unaccustomed to pure or simple tones, since they are generated only in unusual situations, but it is thoroughly accustomed to the various natural overtone clangs and compounds in speech, noise, and music. Whether this habitual perception of 'oneness' is merely carried over, or whether the physical rôle of overtones in fusion is the determin-

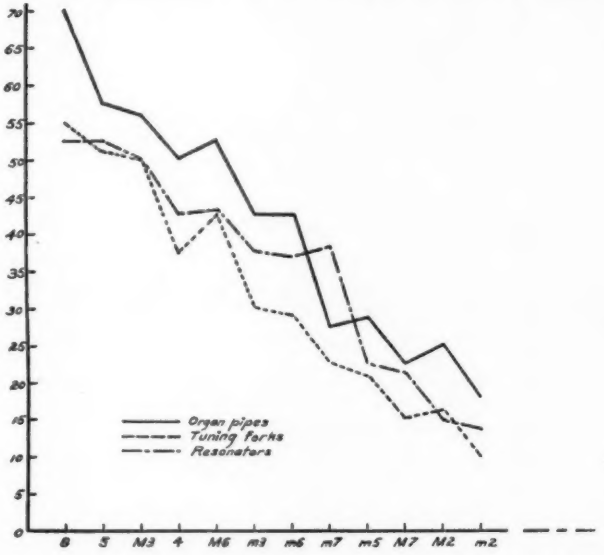

FIG. 5. SMOOTHNESS DATA FOR GROUP II

The ordinates represent the percentage of total trials in which an interval is called "smooth." The abscissae represent the musical intervals

ing factor, the results are nevertheless evident and consistent. Since, however, there is not coincidence in amplitude for the fork and resonator series, both of which produce pure tones, it is desirable as a future study to determine the exact influence of intensity by using generators of minimal and of maximal intensities.

As would be expected, the difficulty or ease of judging the number of tones generated, as indicated by plus or minus, is almost directly proportional to the degree of sensory fusion.

Fig. 10 shows the curves in terms of plus-judgments for all the groups; the minus judgment curve is merely inversely proportional to the plus-judgment curve. The reduced amplitude of the plus curve is due obviously to two types of 'error within error,' which are discounted in the averages. For instance, a trial in which the judgment of an octave is recorded 1+ is omitted in the computation, and only correct combinations, such

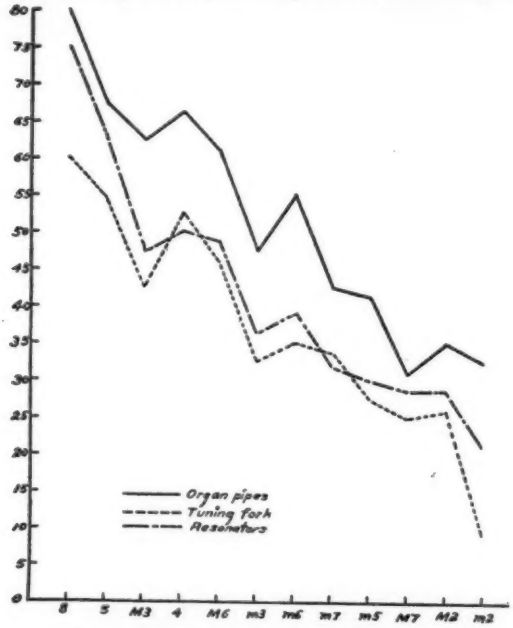

FIG. 6. SMOOTHNESS DATA FOR GROUP III
The ordinates represent the percentage of total trials in which an interval is called "smooth." The abscissae represent the musical intervals

as 2− for a major fifth, or a 2+ for some other interval of two primaries, are used for statistical reasons. Similarly, a judgment of 4+ or 4− in response to any interval is discounted, since, for the purpose of this particular curve, one would cancel the other. In no instance, as a matter of fact, are more than two tones sounded simultaneously, though there is a plentiful sprinkling of unisons used for checks.

Fig. 11 shows the curve for these 'errors beyond two.' It is readily seen that the tendency to overestimate the number of

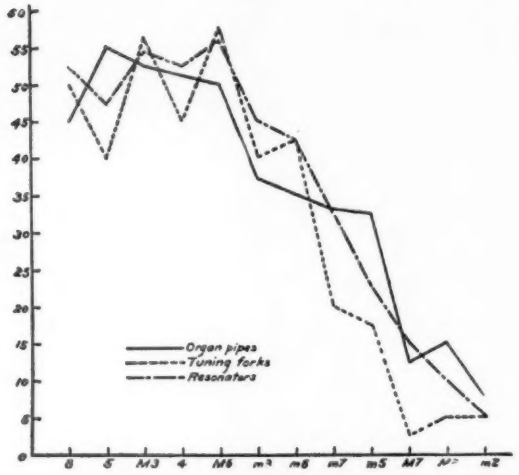

FIG. 7. PLEASANTNESS, GROUP I

The ordinates represent the percentage of total trials in which an interval is called "pleasant." The abscissae represent the musical intervals

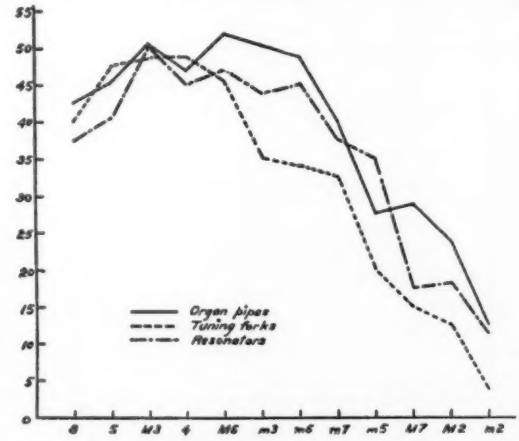

FIG. 8. PLEASANTNESS, GROUP II

The ordinates represent the percentage of total trials in which an interval is called "pleasant." The abscissae represent the musical intervals

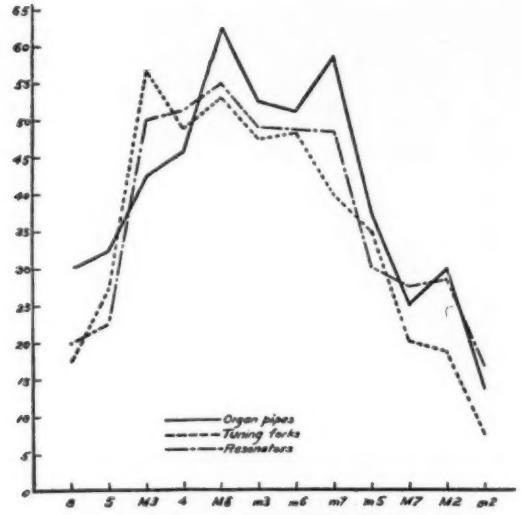

FIG. 9. PLEASANTNESS, GROUP III

The ordinates represent the percentage of total trials in which an interval is called "pleasant." The abscissae represent the musical intervals

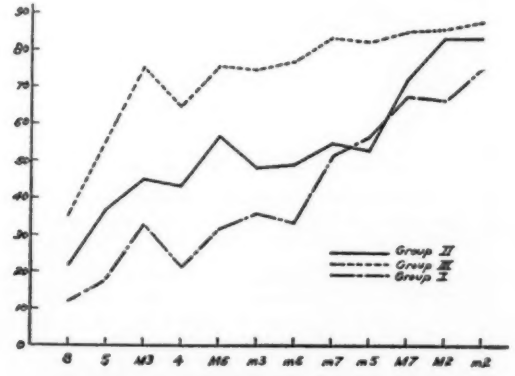

FIG. 10. PLUS JUDGMENTS. RESONATORS

The ordinates represent the percentage of total trials in which the subjects recorded their judgments as being easily made (judgments of one or more tones). The abscissae represent the musical intervals

sounding primaries varies proportionately with decreasing sensory fusion. For instance, the weighted curve for all groups shows the following table:

Interval:	8	5	M3	4	M6	m3	m6	m7	m5	M7	M2	m2
Number 'multiple' errors:	6	14	20	18	18	20	29	33	34	41	45	47

With tuning forks and resonators these multi-tone judgments result primarily from thirds, sixths, sevenths, and seconds. Although the curve for organ pipes follows the same general contour—all its intervals are more frequently confused

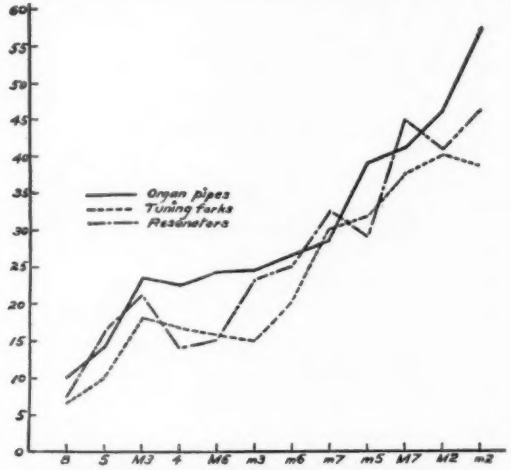

FIG. 11. MULTIPLE TONE JUDGMENTS, GROUP I

The ordinates represent the percentage of total trials in which the subjects recorded judgments of 3 or more tones. The abscissae represent the musical intervals

by many *Ss*. In the first case, difference and summation tones may possibly be held accountable for the results, although this assumption would belie the fact that difference tones from the simple fusions are ordinarily more easily audible than those from dissonant primaries. Certainly in the latter instance, the multiple auditory effect may be the natural consequence of overtones.

The technique of successively actuating and selectively damping the tuning fork series leads to confusion of judgments.

Ss in general report a carrying over of auditory after-images. Most of the *Ss* in Group III remember all the tones sounded, and recognize each as it is eliminated. This 'confusion' on the part of the untrained *Ss*, and the analysis on the part of highly trained *Ss* doubtless accounts for the somewhat erratic variations from those curves established by the more direct and simple method.

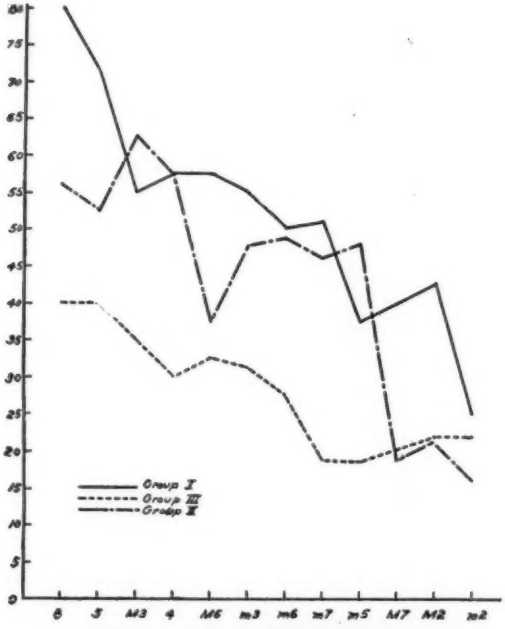

FIG. 12. FUSION: TUNING FORKS
GROUP ACTUATED AND SELECTIVELY DAMPED
The ordinates represent the percentage of error (two tones heard as one). The abscissae represent the musical intervals

The series of single-tone stimuli are introduced chiefly as a check against practice and attitude, and serve excellently as an illustration of the effect of *Aufgabe*. The directions given for these series are identical with those given for the other fusion series. Curves for Group I and Group III are in terms of absolute error, and while their form apparently means very little, the difference in amplitude is again strikingly indicative of variations in acuity between the two groups of *Ss*. This

compound perception of a 'simple' stimulus must be accounted for either by the presence of audible partials or by the carrying-over of auditory imagery. The first effect would be expected where pipes are used, but is theoretically inexplicable in the case of pure tones, without the assistance of auditory imagery, tonal memory, and habituation reactions.

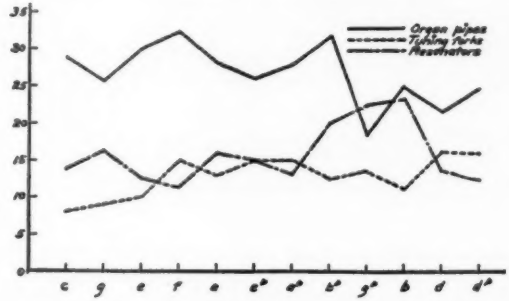

FIG. 13. FUSION: SINGLE TONE STIMULI, GROUP I
The ordinates represent units of absolute error (*i.e.* single tone stimuli judged as two or more). The abscissae represent the tonal series used

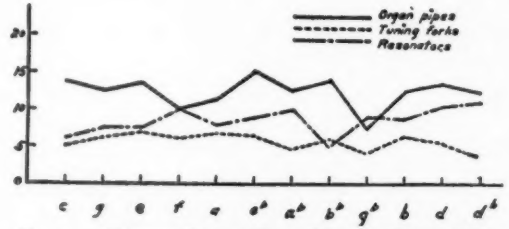

FIG. 14. FUSION: SINGLE TONE STIMULI, GROUP III
The ordinates represent units of absolute error. The abscissae represent the tonal series used

The influence of sequence is apparent in several ways. A unison following another interval than an octave is frequently called an octave, and vice versa. An octave is much more likely to be called 'one' tone if preceded by a second or major seventh than if preceded by a fourth or fifth. Similarly, the interval of the second following an octave or a unison is practically always perceived as two, and frequently as four or five. The effect of sequence is not only in contrast, but is apparent in auditory after-images and auditory memory. The longer the

time interval between the successive clangs, the less the effect of previous stimulation and affective inertia on the specific judgment. This is, however, partly dependent on the nature of the individual listener; *Ss* with pronounced auditory imagery are more prone to direct comparison and contrast than those with poor auditory imagery. These subjects frequently report a difficulty in obeying the instructions to 'isolate' each disparate stimulus as such.

By reference to Figs. 7, 8, and 9, the striking differences in affective response among the three groups of subjects are immediately obvious. Using the resonator series as a standard, the following table may serve to illustrate these variations:

Preferences in terms of degree of Pleasantness

Group I:	M6	M3	4	8	5	m3	m6	m7	m5	M7	M2	m2
Group II:	M3	M6	4	m6	m3	5	m7	8	m5	M2	M7	m2
Group III:	M6	4	M3	m3	m6	m7	m5	M2	M7	5	8	m2

Two outstanding features of the various curves are the sharp decline of the minor second, and the high affective value of major thirds and major sixths. Among Group III the octave and fifth are relegated to a surprisingly low point, even the major and minor seconds and sevenths superseding them in affective interest. In the original records, octaves and fifths are most frequently called 'indifferent;' minor seconds are called 'unpleasant,' a difference in reaction which is highly significant, even though it cannot be incorporated in a curve built on 'pleasantness' judgments alone.

Conclusions

From analysis of the preceding facts, the following conclusions apropos of consonance seem justified:

(1) Consonance, if kept in strict accordance with its literal definition, cannot be defined in terms of either of the two criteria, fusion or smoothness.

(2) Tonal fusion, as such, is a purely sensorial phenomenon, perfectly distinct from the aesthetic or affective experience of consonance. This statement must be qualified, in that the lower stages of consonance and the lower stages of fusion almost universally coincide. The one marked example is the minor second. With the higher stages, there is not coincidence, but neither is there antithesis. For example, the octave is in few instances called unpleasant, but is quite generally indifferent.

(3) Smoothness, minus a special arbitrary interpretation on the part of the instructor, is equally insufficient as a criterion, denoting as it does, manifold connotations and depending obviously on the musical experience of the individual.

(4) Of the three criteria, the affective elements of pleasantness and unpleasantness seem most legitimately applied to consonance, and most competent to account for the historical shift in consonant intervals, and the personal idiosyncracies in harmonic judgment.

(5) Differences in the perception of consonant intervals is dependent more on the mental setting, *i.e.* the criteria of judgment and the musical nature and training of the individual, than upon musical differences of quality in the generating tones, or the absolute or relative intensity of the stimuli. Fusion and smoothness on the contrary are definitely conditioned by intensity, clang color, duration, time sequence, and all the other variables hitherto mentioned in connection with technique.

(6) In accordance with Max Meyer, it would appear from this study that consonance and dissonance are not antonyms other than as they apply to muscial tones and to noise. One universal exception to this conclusion is the minor second, which holds the lowest point on all the curves derived from these experiments.

Discussion

As has been repeatedly implied, the significance of consonance, fusion, smoothness, in practical music, is a relation stated by many, and explained by few psychologists and aestheticians. Stumpf was the first psychologist to suggest that musical systems had their psychological roots in tonal fusion. Helmholtz considers the 'dissonant' intervals, or those involving beat phenomena, aesthetically unpleasant, and he regards the music of Wagner and Berlioz, for instance, as inferior to the simpler and smoother music of Haydn and Mozart, or even to the earlier Palestrina forms of polyphony. He says, to quote him directly, "that expression which modern music endeavors to attain by various discords and an abundant introduction of dominant sevenths, was obtained in the school of Palestrina by the much more delicate shading of various inversions and positions of consonant chords. This explains the harmoniousness of these compositions, which are nevertheless full of deep and tender expression, and sound like the songs of angels with hearts affected but undarkened by human grief in their heavenly joy. Of course such pieces of music require fine ears both in singer and hearer, to let the delicate gradation of expression receive its due, now that modern music has accustomed us to modes of expression so much more violent and drastic."[54]

Even musicians of the present day seem prone constantly to refer to Bach and Haydn or the early church chorales as 'pure'

[54] Helmholtz, *op. cit.*, 225 f.

music, thereby implying that contemporary music is 'impure,' contaminated by intervals other than the octave, fourth, fifth, and sixth.

Külpe likewise lays the psychophysical foundations of musical forms in consonance, even though he can give no final accounting for that term. He says that polyphonic music makes a more or less sharp line of distinction between consonant and dissonant compound clangs, and that it is reasonable to suppose that this distinction is based upon a definite psychological effect.[55]

Coming down to modern texts in harmony, counter-point, and fugue, we find the following familar 'laws' which wield theoretical absolutism, at least, over musical composition. Thus in Shepard,[56] "intervals are classified according to their musical effect, as (*a*) *consonant*, meaning those intervals upon which it is agreeable to pause, and which do not need to be followed by another interval to produce a pleasant effect; (*b*) *dissonant*, those not satisfying to dwell upon, or to use in the final chord of any composition. Being a dissonance, the two notes of the interval of the second and seventh should not be sung together, unless once or twice merely to show their dissonant character."

Similarly, we find in Richter's popular text, the statement that "a consonant interval is one whose tones are in so pure and satisfactory relation as to require no further special connection with any other intervals as a necessary completion of their harmony."[57] "Diminished primes, (major sevenths), seconds, and ninths, are harmonically inconceivable. The diminished sixth, like the augmented third, may occur only in exceptional cases, in which the perfect fifth may generally take its place, as the perfect fourth may be substituted for the augmented third."[58]

To an obedient composer, therefore, dissonances are restricted to melodic progression or else to mere passing notes.

It is further significant, that in our inherited musical vernacular, the octave, fifth, and fourth are classified as *perfect* intervals, by virtue of their perfect or complete consonance, whereas for thirds and sixths, whose consonance is 'incomplete' (whatever that may mean), the ordinary terms, major and minor, must suffice.

There is, of course, no agreement as to the origin of music. Max Meyer,[59] and Lipps,[60] defend a melodic basis; the majority of psychologists and aestheticians, however, seem either to accept or to modify[61] Wallaschek's derivation from the rhythmical

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[55]Külpe, *op. cit.*, 280 f.
[56]F. H. Shepard, *Harmony Simplified*, 1912, 35.
[57]E. G. Richter, *Manual of Harmony*, 1912, 4.
[58]*Ibid.*, 3.
[59]Meyer, *op. cit.*, 2.
[60]Lipps, *op. cit.*, 131-135.
[61]Richard Wallaschek, *Primitive Music*, Chap. ix.

impulse in man. Few have, however, attributed the origin of music to an 'impulse of consonance.' Yet the Hottentots, the Bechuana, and the Bachapin tribes of Africa were found to have gom-gom orchestras which played in parts,[62] and to sing their monotonous, undeveloped melodies of two or three successive tones in harmony. In Java, orchestras with native instruments play variegated and rather elaborate music in different parts, and though the harmony may sound odd and unmusical to our ears, it indicates, under analysis, an approach even to the intricacies of counterpoint. In Siam, appreciation of concords is highly acute and the Siamese are capable of tuning almost any instrument, native or foreign, to a highly exact pitch. The New Mexican Keres sing three-part songs, and in Guatemala primitive marimba orchestras play ensemble and accompany songs. According to Wallaschek, these ethnological facts refute the traditional theory that harmony is a modern or even a recent invention. He would make it coincidental with melody and both of them subsequent to rhythm.

Whatever the origin of music, the following facts stand historically in relief: (1) A narrow, but nevertheless legal majority of primitive peoples prefer to sing or perform in unison. (2) The intervals of the octave, fifth, and fourth are, with a few exceptions, common to practically all nations. One striking exception is the highly developed Javanese music, which is lacking in fourths, leading notes, and also in the semitones. On the other hand, in some primitive scales, the fourth is much more prominent than the fifth. (3) Major and minor scales and harmonies occur with about equal frequency in primitive musical systems.

Consider the structural development of musical categories. The definite harmonic system which we have inherited, is obviously based on the scale which we have adopted. Our diatonic scale, in turn, is a heritage from the Greek tetrachord which used no tonic or keynote, and the Greek scale in its turn, represents a definitely melodic or homophonic musical era.[63] According to most authorities, natural differences in vocal apparatus led to the repetition of ceremonial melodies an octave, a fourth, or a fifth below. Then came the descanters, providing two distinct melodies and the real germ of polyphony. In the early Reformation, chorales involving 'tonality,' that is, key signature, influenced church music, and practically standardized the modern diatonic scale in its major and minor modes, which is based on the relation of consonant intervals. Bach, in turn, standardized the system of tempered intonations, and our musical expression since that time has continued in these categories.

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[62] *Ibid.*, Chaps. i-iv.

[63] Waldo S. Pratt, *History of Music*, 1911, Chaps. i and xv.

To present condensed history in another way, early composers of one European generation looked on music from a modal point of view, that is, continued the Greek and Roman emphasis on melody and scales, the sequence of single tones being the important element. From this historical circumstance we have our innumerable laws of melodic progression.

From Bach (1600), to the later Wagnerian era, composers looked on music from a diatonic, that is, a harmonic point of view. The restrictions of the diatonic system are apparent. It uses only two scales out of twelve, it employs definite key signatures and insists upon them, for finality.

With Wagner was introduced the real beginning of chromaticism, and with it, our present musical revolution and its corresponding hiatus between consonance and dissonance; likewise, the psychological feud between the musical eclectics and the insurrectionists. The chief element of chromaticism is condensation, *i.e.* the omission of connecting links which the ear has come to recognize. Consequently, many of the unfamiliar harmonic intervals and sequences are really by-products of this tendency. In addition to chromaticism, there are the following definite revolutionary phenomena in modern music—the practical existence of which is quite enough to render obsolete any standard harmony text, and most of the contemporary psychological theories of music:

(1) Sequence of sevenths, both diminished and major.
(2) Use of consecutive octaves and fifths.
(3) 'Free' resolution of discord.
(4) Abolition of key signature and tonal centers in composition.
(5) Abandonment of the dominant chord.
(6) Juxtaposition of chords not in the same key.
(7) Tendency to eradicate the whole diatonic system in favor of a 'pure temperament' 12-note scale, or a quarter-tone scale.
(8) The use of discords obviously for their own value, and not as mere contrast foils.

Hitherto the whole history of musical composition has presupposed chord-formation of which one consonant interval, at least, would form some part. Yet even the first and ninth symphonies of Beethoven enter on dissonant keys. In certain parts of Brahms' famous *Requiem* we are led from one dissonance to another, never arriving at the expected resolution. Even eminent critics of the time described *Tannhäuser* as a commonplace display of noise and extravagance, as shrill noise and broken crockery effects.[64] The history of musical biography is, in fact,

[64]D. C. Miller, *The Science of Musical Sounds*, 1926, 23 f.

a history of the struggle of the great composers to establish harmonies and rhythms against contemporary prejudice. Bach, Beethoven, Liszt, Schumann, all used dissonance for 'color.' Today we hear the principal theme in Stravinsky's *Le Sacre du Printemps* heralded fortissimo by trumpets in major seconds. Debussy, who has already become an accepted classic, writes in a whole-tone scale and uses sequences of major seconds and sevenths with no compunctions of conscience. Richard Strauss, in the *Domestic Symphony*, uses a combination of strong dissonances with simple strains. In Gustave Holst, we hear constantly and startlingly the juxtaposition of different tonalities, at times even a superimposing of different keys. Schoenberg completely telescopes our ordinary musical perception by a chronic ellipsis of all familiar points. *Black Roses* of Sibelius is really a succession of minor common chords, and the theme of his *Fourth Symphony* makes frequent use of the minor second. Minor seconds occur with striking effect in Bantock's beautiful cantata, *Christ in the Wilderness*, though their consummate master is Ravel in *Pavane* and *Shéhérazade*. Rimsky-Korsakov used dissonant chords, as he says 'for color,' but nevertheless leaves his audience frequently dangling in hopeless expectation of a consonant resolution.

Scriabin, who is perhaps the real revolutionary prophet in modern composition,[65] gives a completely new system of harmony, abolishes major and minor modes, annihilates modulation, and chromatic inflection; abandons all key signatures, and even establishes that most 'ultra-modern' of scales, the duodecuple. His favorite chord, one which gives a peculiar affective color to all his music, is a dominant 13th with a flattened fifth and a major ninth. He also uses chords of two unequal fourths, and innumerable major seconds, though these latter occur chiefly in suspensions. The naming of modern musical iconoclasts might go on indefinitely—Honegger, Casella, Mossorgsky, Dukas, Milhaud, and others have sought their free and individual paths in all the legitimate and illegitimate permutations of musical sound. We have as a result, a music suggesting the rugged, the grotesque, the exotic, the startling; and it is admittedly a contrast with the smooth, sophisticated, sinuous, romantic music to which we are accustomed. Melody has remained more or less immune from the cataclysm. The radical innovations have occurred in harmony, rhythm and instrumentation.

It is primarily at this point where the conundrum of consonance occurs. In the orthodox and scientific sense of the word, this music is not music, but dissonance. It represents a

[65] A. E. Hull, *Modern Harmony*, 1915, 72.

definite existing musical 'cult of the second' in which discord is its own excuse for being. Secondly, this dissonance is largely ridiculed by the musical laity, who quite reasonably claim that they cannot understand it. But whether or not they understand it, the fact remains that the best symphony orchestras play it, the best concert artists perform it, and the best teachers in the best conservatories teach it. Furthermore, the reason given by artists and teachers for encouraging it, is that they like it, that it is music of a very high order, and that it releases the whole field of musical aesthetics from the arbitrary consonance-dissonance shackles of the past.

On the other hand looms the evidence from physics, physiology, psychology, ethnology, and aesthetics. Physics asserts that the simplest ratio between ether vibrations gives the best and simplest fusion, and that this fusion corresponds to the 'natural' overtones of any generator. Physiology asserts that intervals with simple fusion are perceived with less difficulty, are more naturally attuned to the aural apparatus, and are therefore less fatiguing and injurious to the sense organ. Psychology asserts—or at least the 'furtherance-hindrance' theory does—that in general, simplicity of perception, undisturbed flow of nervous excitation, lack of fatigue in the sense organ, give rise to the affective concomitant of pleasure; that contrary conditions, complexity of perception, unrhythmical, interrupted flow of nervous impulse, fatigue, and injury in the sensory ending create unpleasantness. Ethnology asserts that the octave, fourth, and fifth are common, practically, to all music, though a few nations, and many children, regard seconds and sevenths with equal affection. History demonstrates the unison and octave as the original material of music, with the subsequent addition of the fourth and fifth, the still later addition of the third and sixth, the almost contemporary adoption of the diminished seventh, and the purely contemporary tolerance toward the second and major seventh.

It remains the difficult task of the aesthetician, who in this case must be primarily a psychologist, to arbitrate this feud of inconsistency. The solution herewith appended is no panacea, but strictly a mongrel and a compromise, based on the observable facts at hand. It would read as follows:

(1) Consonance is an aesthetic description, totally dynamic in nature, and is not a scientifically determinable constant.

(2) Its perception is conditioned by (a) natural sensory processes, within very wide limits, and (b) the empirical factors of training, environment, musical context, etc. The lower limit of these sensory processes is represented by the minor second, which appears to be universally unpleasant, even with

highly trained *Ss*; and any smaller intervals potential in quarter-tone and eighth-tone scales which incur a *consciousness* of beats. These beats may not be in themselves unpleasant, but they distract attention from the sound as such and consequently detract from the purity of the tonal experience. They are, in fact, directly comparable to the pulsations of the lowest tones from pipe organs, which, if unrelieved over a long period of time, become an excessive strain and inhibit the thematic impressions of the music. If the objection be raised that these beats disappear under conditions of monaural clang perception, it may be stated in turn that without radically different evolutionary tendencies or the universal indulgence in some sort of intricate aural contrivances, sound and music will continue to be perceived binaurally. In light of present evidence, however, it is possible that, could its beats be somehow filtered out, the interval of a minor second might very well become a consonance.

(3) Fusion is apparently a simple sensory process, and on the basis of the Helmholtz theory in particular, may be legitimately classified as perfect, imperfect, and very imperfect. It may be a criterion of smoothness, and vice versa, but not of consonance, unless the real derivation and meaning of consonance is in turn arbitrarily identified with simple sensory summation. Fusion, apparently, would fit Malmberg's definition as a "purely cognitive process,"[66] whereas consonance will not. Not only the data from extensive experimentation, but the voluntary negations and involuntary admissions (witness the number of times the investigators speak of dissonance as 'unpleasant') would indicate that feeling cannot be outlawed as a major component of consonance. The romantic and even political struggle of the major and minor thirds and sixths toward their present high place in our affection, to say nothing of our frequent song-endings in the plaintive diminished seventh, are indisputable evidence for the contention that what we regard as pleasant we also regard as consonant. This attitude in turn, is a composite social heritage from our entire racial past, and our entire musical present, and is aesthetically and psychologically legitimate so long as its expression does not transcend obvious physical and physiological limitations.

Musical theory and aesthetic principle, as well as the psychology of music, are hence confronted with difficulty in fundamental terms. If we speak of 'dissonant' music, we are really combining antonyms; if we speak of 'consonant' music in the traditional, narrow sense of the term, we have left no name for the bulk of composition since Wagner's *Tannhäuser* overture set its first audience hissing. The proper compromise therefore,

[66]Malmberg, *op. cit.*, 131.

would seem for us to revert to linguistic purism, and classify consonance, always historically, in terms of its original synonym, *i.e. harmony*. Hull,[67] who is prominent among muscial theorists, states definitely that all the textbook laws of harmony must be modified in this respect. If musicians are willing to remold their traditional architectural structure, psychological theories of music must likewise concede expansion of their terms and tenets.

The import of this discussion, applied to practical music, is tolerance. Tolerance ordinarily leads to interest, interest to knowledge, and knowledge to a catholicity of taste. This tolerance must at the same time be intelligent enough to recognize that these new harmonic values exist because they have evolved or diverged from known values, and that evolution does not imply discarding the classic for the new, though it may represent a shift in affection. It is not that we fail to understand the simple 'consonant' music of Haydn or Palestrina, but that we, as modern listeners, prefer in general the rich complexity and emotional vitality of Tchaikowsky or Rachmaninoff as more in harmony with the complex texture of our civilization. 'Ultra' modern composers have merely stretched their affections one degree farther, to include the kaleidoscopic subtlety and diffuse tone color of hitherto forbidden intervals. Judging them by their own standards, which are obviously not the old, inadequate 'laws' of consonance, we may conclude that 'dissonance' is reduced to consonant relativity, and their music violates neither psychological principles nor aesthetic standards.

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[67] A. E. Hull, *Scriabin*, 1916, 255.

MEANINGFUL BEHAVIOR IN HYPNOSIS

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Broadly speaking, the stream of present-day psychology flows along two channels, traditional academic psychology and medical psychology. The academic psychologist recognizes, or should recognize, that his basic concepts must be adequate for the description and explanation of all the facts, normal as well as abnormal. The logical position of the medical man is the same. Accordingly we find each of the contending parties claiming the entire field. On the one hand, medical psychology rejects the basic concepts of academic psychology as inadequate and sterile; the academician, on the other hand, brands the fundamental ideas of mental medicine as obscure, illogical and unscientific. In the present article I do not wish to become involved in this controversy. None, except possibly some academicians, seriously questions the immense importance and significance of the medical contribution. On the other hand, I must confess to that infirmity of small minds, the love of consistency and clearness of thought. Unconscious or subconscious knowledge, unconscious desire, repression, dissociation—somewhere behind these labels there are facts of profound significance. The facts themselves are neither logical nor illogical, neither clear nor obscure, neither consistent nor inconsistent with other facts—they are. But the clearness and consistency of our description is the measure of the intellectual control which we have over the facts. In the present article I shall try to make a contribution toward the problem of gaining intellectual control over the facts which medical psychology has brought to our attention.

The facts with which I shall deal are the facts of hypnosis. The basic descriptive and explanatory concepts which I shall utilize were presented by me in two previous papers.[1] In order to facilitate that utilization, it will be convenient to refer briefly to some of the ideas expressed in them. In these articles I did not offer a new solution of the old conventional problems. I stated a new problem. In all of academic psychology *meaning* is very troublesome. The objectivist relegates it to the limbo of 'subjective' fact and does not deal with it at all except perhaps to speculate about its 'objective' correlates. The dualist who follows the prevailing fashion of dealing with facts of be-

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[1]Curt Rosenow, Behavior and conscious behavior, *Psychol. Rev.*, 30, 1923, 192-216; The problem of meaning in behaviorism, this JOURNAL, 36, 1925, 233-248.

havior and with facts of consciousness, explicitly recognizes meaning as an attribute of mental processes, or as an elementary mental process, an imageless thought, an *Aufgabe*, or the like. There has been some discussion of what meaning *is*. There has been some discussion of the meaning of meaning. The purposivists have preached that our descriptions of mental processes are vitiated by a neglect of the purposive aspect of mental life. Meantime there are countless human activities which undoubtedly mean something, but the meaning of which is unknown. In many of these cases the meaning is unknown even to the actor. But so far as I know, no academic psychologist has envisaged that state of affairs as a psychological problem. In the articles referred to above, I have called attention to that problem. I have discussed a psychology which sees its major problem to be *the discovery of the meaning of activities in cases where that meaning is unknown*. Furthermore, I have endeavored to point to the path which he who is interested in the solution of such problems should follow.

So far as psychology is concerned, that path has been obliterated by the fact that psychologists have taken for their point of departure positions which are essentially metaphysical positions. In many cases that is due to lack of familiarity with, or lack of understanding of, the history of philosophic thought. In few cases have these fundamental positions been chosen with adequate appreciation of their logical implications. In any case the result is that the technical terms of psychology (and behaviorism) are saturated with metaphysics. To escape from these metaphysical difficulties which, it seems to me, are irrelevant to the genuine problems of psychology, I fall back upon ordinary language, taking care to limit the connotations and denotations of a few terms in such a way as not to become involved in the conventional puzzles and contradictions. The next seven paragraphs are devoted to this task.

The idea of an incomplete act has reference of some kind to what the act would be, or would have been, were it to be completed, or had it been completed. The psychologist who speaks, glibly enough, about nascent or covert activity or about implicit behavior is urged to take note of this simple, obvious fact. Thus if I start to look at X but change my mind, the psychologist may, if he chooses, describe this event as 'nothing but' an eye-movement. But he should make it clear to himself (and to others) that the eye-movement has reference of some kind to *looking-at-X* even though, as a matter of fact, I did not look at X. Failure to do so will leave him without defense against the charge that he *pretends* to be able to understand nothing but muscle twitches. For it is impossible to believe that the behaviorist is as stupid as he pretends to be.

I use the verb 'to mean' as a general term designating a very large number of specific activities, just as the word locomotion is used to designate swimming, walking, running, etc. Just as nothing is added to the description of swimming by saying that the organism is locomoting, so nothing

is added to an adequate description of the activity of planning the next move at chess by saying that it is a meaningful activity. Just as an individual is locomoting only while he is swimming, walking, or running, so the individual means (*e.g.* to answer the door bell) only while he is acting. Up to this point in the argument this use of the verb 'to mean' is taken from ordinary usage without modification or addition. I recognize of course that the word is in current use in other ways as well, but in order to secure clarity of thought I limit my use of the term as indicated above. The reader who cares to make the effort necessary to the understanding of this position *must* do the same. There are many synonyms such as indicate, imply, signify, denote, connote, etc., which make the procedure quite feasible. It is easy to say that the east wind signifies rain in order to avoid the ridiculous implication that a phenomenological entity, the east wind, is engaged in a meaningful activity.

The question "What does a given specific act mean?" is to be answered on the basis of relevant evidence. Thus, if the first man at bat makes what looks like a base hit and runs toward first base, the evidence supports the view that he is trying to get to first base before the ball. Of course that may be a mistake in any given instance. There are dishonest players. I assert merely that it is *psychologically* possible for human beings to mean to do things and that it is possible sometimes to secure evidence which enables us to state with considerable certitude what an act means or has meant. A psychology which makes it impossible to ask and answer questions of this character bears witness to its own sterility. On the other hand there is to be no a priori assumption that all living activity or all human activity is meaningful. What does an oyster mean when it opens its shell? Is the growth of the fingernails an activity of the living organism and, if so, what does it mean? In the absence of relevant evidence, we can dispense with cheap and easy generalizations which answer such questions. We do not know what, if anything, such activities mean—and that is the only honest answer which is possible at the present time.

With reference to nouns in common use, such as experience, knowledge, awareness, and the like, I note that ordinary speech frequently uses these nouns merely as convenient substitutes for the corresponding verbs which denote activities such as experiencing, knowing, and the like. Indeed common sense has no more difficulty with these terms than the physicist has with the word motion. But during the history of thought a distinction has arisen between the activity of experiencing and that which is experienced. Since the distinction has been made, the nouns have become somewhat ambiguous. In that state of affairs, the least that can be demanded of the psychologist is that he avoid this ambiguity in some way. Instead of that psychologists have achieved the following logical atrocity. Beginning with the distinction between the activity of experiencing and the phenomenon experienced, they have 'discovered' that the experienced phenomenon is itself a mental process. Accordingly we have our choice of two alternatives. In a concrete case where, *e.g.* an apple is seen, either the apple or the seeing-of-the-apple is the experienced phenomenon. In the latter case the distinction with which we started has somehow disappeared. In the other case we have one mental process, viz. the activity of seeing, somehow related to another mental process, viz. the seen apple. Here the distinction with which we started has been preserved at the expense of intelligibility. In order to avoid puzzles of that kind, in order to avoid ambiguity, I shall confine myself to the use of the verbs such as experiencing, knowing, etc., and when I do use the nouns it will be as a convenient substitute in order to secure ease of diction. Anyone who is interested in the problem of ultimate reality must face these problems which I am attempting to avoid, but, as I see it, that is not the task of the psychologist. It is legitimate to orient oneself toward the modern science

of physics, but that orientation will prove a very easy matter if neither the physicist nor the psychologist insist on attacking the problem of ultimate reality.

In common with all biological science, I envisage an organism reacting to an environment. In common with these sciences, in describing the various relations of an organism to its environment, I describe facts and conditions not dependent in any way upon the meaningful activities of the particular organism in which I happen to be interested. Thus the presence of a stick of dynamite with a lighted fuse attached is important especially in the absence of any meaningful reaction to it on the part of the particular organism I happen to be observing. In the same way, I describe intraorganic facts and activities the meaning of which, if any, is unknown to me. Thus the development of a cancerous growth may be of importance and may need to be described. In addition, I ask, "What, if anything, do the activities of this organism mean?" and, so far as the evidence permits, I attempt to answer the question. It is possible to omit all reference to the meaningful activity of the organism, but description of this sort would be excessively cumbersome and, as a matter of fact, is never attempted in the case of highly developed organisms. Instead the objectivist uses such language as will serve his purpose, declaring that he uses that language in an 'objective'sense only. He may speak of a dog running after a rabbit, but he means to convey merely that the dog is running in the same direction as the rabbit. Such procedure is legitimate enough provided it is a genuine problem whether that particular dog is running after that particular rabbit, but if it is held that it is theoretically impossible for dogs to run after rabbits and that that popular illusion must be replaced by 'objective' description, we are dealing with a crude, antiquated form of metaphysics and may leave the amateur metaphysician to his not very intellectual devices. Every useful purpose served by the distinction between the objective and the subjective can be attained by substituting the distinction between the given and the problematic. By the term 'given' I mean that which I believe to be true, that which I can support by strong evidence, and that which I wish my reader to take for granted. The investigator who is dealing with a genuine problem and who has described his data in such a fashion need not bother with the distinction between the objective and the subjective.

Thus far we have not gone beyond the confines of ordinary language. We have limited a few terms, but we have not extended their significance beyond accepted usage. We enter now upon more questionable ground. I leave it an open question whether I use the verb 'to mean' as a synonym for the word 'purpose,' and I do not care to embark, in the present article, upon an exhaustive discussion of the latter term. Nevertheless some discussion will be useful. One of the chief difficulties we encounter is whether the organism which acts purposively 'knows' the end or purpose of its own act. If we deny it, we seem to become involved in a contradiction in terms. If we affirm it, we are confronted by instances where that contradiction appears actually to happen. We can of course take refuge in 'unconscious' purpose, but that is merely the fashionable way of admitting that we cannot resolve the intellectual difficulty and have found the effort wearisome. In this dilemma we may appeal to the genius for the concrete of William James. In discussing the broody hen, William James said that we must suppose that a nestful of eggs to a hen is the most interesting, never-too-much-to-be-sat-upon object in the world. It seems to me that this flash of insight will solve our problem. We must bear in mind that the behavior of the hen is not language behavior, but that we must describe it by means of language. The appearance of the eggs is not a fact of language, but, described by means of language, the eggs look like never-too-much-to-be-sat-upon objects. The fact that the hen-sees-the-eggs-in-this-fashion and the fact that the hen-means-to-sit-upon-the-eggs is one and the

same phenomenon. Accordingly I assert that the activity of knowing, experiencing, or awaring (pardon the neologism) and the activity of meaning (*e.g.* to sit) is one and the same activity. Most of our meaningful acts are never completed but mean their tentative completion. But this tentative completion is seen, heard, smelled, or felt. There is to be no disjunction between that which is seen (perceived, known, meant) and the activity of seeing it. The-hen-sees-never-too-much-to-be-sat-upon-objects; that *is* the act. Obviously the eggs are never-too-much-to-be-sat-upon objects only with reference to the acting hen. Considered as a part of the environment they can and should be described in other ways as well, but the existential status of the never-too-much-to-be-sat-upon objects (as such) is not a problem for psychology—neither is the existential status of the eggs. It is only by avoiding this disjunction, as ordinary usage avoids it, that the psychologist can avoid the logical obligation of embarking upon the troubled seas of ontology and epistemology. Indeed I have joined only what no psychologist should have put asunder.

It should be obvious to the psychologist that not all individuals are able to describe what they experience with equal skill. As a limiting case we have the very young infant who cannot describe at all. Moreover, the descriptive ability of the adult varies with circumstances. There is, for example, the well-known case of the man who has just looked at his watch but is unable to say what time it indicated. In many cases I have been able to restore the 'lost memory' somewhat as follows. I recall that I looked because I had an engagement at three-thirty. I remember that I found that I had plenty of time—about forty minutes. Then I remember quite vividly the position of the hands of the watch—two fifty-three. Now I hold that my looking at my watch was the *equivalent* of a question which I asked myself somewhat like, "Isn't it about time for that appointment?" Part of the behavior of seeing the watch was equivalent to the answer, "No, there is plenty of time." A moment later I knew that I had seen that there was plenty of time, but I did not know what else I had seen. Accordingly I distinguish between perceiving and knowing what one perceives. Just as perceiving is an activity, so knowing what one perceives is an activity. I hold with George H. Mead that it is the behavior of acting socially toward one's own activity of perceiving. This point will be elaborated and (I trust) clarified in the body of this paper in conjunction with the facts of hypnosis.

We come now to the topic of hypnosis. I take it for granted that the reader is acquainted with the current theories and with the need which exists for the clarification of ideas. In order to get a list of the hypnotic phenomena before the reader, I quote Bridges as follows.[2]

(a) Rapport, the state of dependence of the subject upon operator. (b) Amnesia in the trance for some past events, hypermnesia for others; and amnesia afterwards for events of the trance. (c) Suggestibility and loss of initiative. (d) Paralysis, contracture, catalepsy, and other effects on voluntary muscles produced by suggestion. (e) Illusions, hallucinations, and delusions. (f) Anaesthesia and hyperaesthesia; negative hallucinations. (g) Post-hypnotic phenomena, suggestions given during the trance are carried out after waking.

These facts are usually explained (or described) through the use of such terms as dissociation, narrowing of the field of per-

[2] J. W. Bridges, *An Outline of Abnormal Psychology*, 1925, 128.

sonal consciousness, repression, the unconscious, suggestibility, and the like. I submit that these terms are utterly unintelligible and, at best, serve only the purpose of superficial description. I propose to discard them all without introducing any terms to take their place. I propose to show that the phenomena are the results of the meaningful behavior of the hypnotized individual in ways which that individual does not understand. As long as the psychologist does not understand them either, he will have need for some of the terms enumerated above. After he comes to understand, he will have no further need of them.

The theory to be put forward is the result of the observation of more than 100 *Ss* during a period of four years. There was no formal investigation. Starting with a rather vague curiosity aroused by the similarity of hypnosis and hysteria, I began to hypnotize volunteer *Ss* and to study them without a well-defined objective. As my ideas crystallized and developed, I found it possible sometimes to modify the conduct of the experiments for the purpose of testing out some definite hypothesis. For the greater part, however, I had to be satisfied with observing the facts as they presented themselves. Within the scope of this article, it will be impossible to present all of my material. It will be impossible also to answer all the questions and reply to the numerous objections sure to occur to the reader who will be unwilling or unable to refrain from translating the language which I use into his particular psychological phraseology. At some time in the future I hope to be able to discuss these topics in the detail which they deserve. At the present time these views are put forward for what they may be worth.

Most of my *Ss* were college students. The degree or depth of hypnosis obtained was usually light. Indeed, followers of Janet would say that most of my *Ss* were not hypnotized at all and would accept only a few of my cases as genuine. Probably Bernheim would have been satisfied with all of them. I shall not concern myself with this ancient quarrel. I observed certain facts and these facts need to be explained. Janet might attempt to explain them by reference to the facile complacency of my *Ss*. I shall, however, let the facts speak for themselves.

I have said that the phenomena of hypnosis are results of the meaningful behavior of the individual in ways which the individual does not understand. Inasmuch as the same thing is true of a great many other phenomena, I wish to present some facts without inquiring whether or not they are facts of hypnosis. The facts may be presented as an account of a class experiment which I perform habitually. The class is instructed to clasp their hands, the fingers entwined as in prayer. They are told to maintain this position and to fixate a point on the blackboard. The position is maintained for about 5 min. and during that time I talk to them about the necessity of maintaining the fixaton. They are to avoid the slightest eye-movement; they are to maintain a clearly focalized vision, and the like. After it has become apparent that it has become very difficult to maintain the fixation, I tell them that they must

maintain it no matter what else I may ask them to do. Then I tell them that they cannot pull their hands apart and challenge them to try. Many of them are unable to do so for a considerable period and nearly all of them experience considerable difficulty in getting them apart. None of them are ever able to give a sensible account of the phenomenon, although those who have heard about suggestion or dissociation appeal to it. Then I tell them to clasp their hands again and to keep them clasped tight at the same time that they are trying to pull them apart. By that time they usually understand that it was the maintenance of a firm grip due to the diffuse tenseness of the difficult visual fixation which was responsible for the phenomenon. They understand that the mysterious character of the phenomenon was due to their temporary ignorance of the mechanical aspects of their own behavior. They knew that they were trying to fixate, but they did not know that they were keeping their hands clasped tight.

So far as I know, this simple and obvious explanation of this familiar experiment has never appeared in print. On the contrary, it is usually performed as an illustration of suggestibility. But suggestion has little or nothing to do with it. In order to demonstrate that I have varied the procedure as follows. I instruct the class that keeping the fixation point at maximal clearness is their chief and all-important task, never to be abandoned for a moment. In addition they are to follow other instructions which I will give them later, but not to abandon their efforts at fixation under any circumstances. When I judge the time ripe I remind them of their instructions and tell them to pull their hands apart, omitting any suggestion that they cannot do so. In my experience the experiment succeeds about as well in this form as when the negative suggestion is employed. That is, about the same proportion of the class encounters difficulty.

Now let us go on to the facts of hypnosis proper. The first thing which should engage our attention is the variability of the phenomena. They vary from individual to individual and they vary with conditions. Some *S*s are very easily hypnotized by almost any method. Others present great difficulty, and it is this type which I have found most instructive. Still others are easily hypnotized by one method but not by another. After hypnosis is induced the phenomena which can be produced vary enormously with the individual and with the method of procedure. I have usually worked with (presumably) normal college students and have employed the method of 'talking sleep,' but have had some opportunity to work with less normal *S*s and have used other methods. There are very many methods

of induction but all of them have certain features in common. I shall base my explanation upon the method I use habitually, but it should become obvious that it applies to all other methods.

The *S* is told that his coöperation and good will are essential to success and then instructed regarding the procedure. He is to lie down on a couch and try to go to sleep. He is to put everything else out of his mind, make his mind a blank, etc. Most *S*s who try to follow these directions close their eyes as a matter of course. If they do not I ask whether it will not be easier if they do, and I have never encountered an *S* who considered it easier to go to sleep with his eyes open. After about a minute I begin to 'talk sleep' in a soothing monotone. It is not my purpose, however, that the *S* should relax completely and really go to sleep. I want him to get drowsy, but to keep on *trying to sleep*. Accordingly, if I find him too relaxed, I raise my voice, or alter my talk, in order to rouse him slightly. I have asked 50 *S*s just how they tried to go to sleep. They replied in a great variety of ways. Some resort to time-honored devices such as 'counting sheep.' Others remark that they try to relax and 'think' of being sleepy. A few say that they find it helpful to listen to the monotonous drone of my voice. Nearly all of them are more or less puzzled by the question. Apparently trying to go to sleep is trying to go to sleep. Not a single one told me that he was keeping himself motionless, resisting most impulses to move, sometimes even retaining obviously uncomfortable positions in order to avoid moving. Yet, obviously, that is what all of them did. In some cases their efforts result in a rigid, mask-like appearance of the face with which all who are acquainted with these phenomena are familiar. I conclude that the *S*s do not know these details of the behavior of trying to sleep at the time they are engaged in this behavior. At the time that the verbal (or ideational) processes mentioned above are occurring, they know that they are trying to sleep, but they do not know these details of their behavior, or, if you prefer, they have only a dim, shadowy notion of them. Inasmuch as I shall make extensive use of this fact, I wish to point out that the same thing occurs in all other methods of induction. For example, in the eye-strain method, the subject becomes tense and motionless, but does not realize (or ceases to realize) that that is part of the behavior of strained visual fixation.

In order to determine whether the *S* is quietly falling asleep or is *trying* to sleep I use various indices. Frequent changes of position are efforts to relax; rigid immobility is what I look for and try to induce. A slighly open mouth indicates relaxation; firmly closed lips, tension. A frown is favorable, a smile un-

favorable. Quiet immobility of the features is ambiguous and hard to interpret unless one has worked with the *S* before. Fluttering eyelids indicate difficulty in keeping the eyes closed, and, consequently, tension; but it may not be wise to suggest that the *S* cannot open his eyes, although individuals differ here as elsewhere. My favorite device is to take a finger and to put it in an unusual position which requires muscular tension for its maintenance. As a rule I support it for a few seconds and withdraw the support gradually and tentatively. If this position is maintained indefinitely, I conclude that the behavior of trying to sleep has eventuated in the maintenance of a diffuse postural tension. If the position is not maintained, I order the subject firmly and commandingly to try to sleep, putting more or less emphasis on the word 'try.' If now the finger is lifted again the position is maintained almost invariably although the time for which it is maintained will vary with the individual. After I have succeeded with the finger I take the arm and put it into a similar position (*e.g.* a horizontal position). Usually the position is maintained as well as in the case of the finger; frequently for 15 min. or more. Now *if* the *S* responds to the manipulation of the arm as a disturbance, *i.e.* as interfering with his efforts to sleep, *if* the response takes the form of increased effort to try to sleep, *i.e.* to hold himself motionless, and *if* that increase of tonus is especially strong in the muscles of the arm on account of the uncomfortable position of the arm, the behavior is accounted for. Failure is explained through the absence of one of these conditions. For example, the *S* responds to the manipulation of the arm as an attempt on the part of the hypnotist to achieve some sort of result and coöperates by relaxing the arm; or else he may become curious and, instead of trying to sleep, he watches what is going on. It may be said in advance of a more thorough discussion of amnesia that *S*s remember (or can be made to remember) after hypnosis most of the incidents which happened during hypnosis. If the position is maintained for any length of time, that incident is, usually, not remembered at all or only very vaguely or uncertainly. When the incident is remembered, some *S*s report that their arm *remained* in the position; others report that they *kept* it there. All of them say that they have only the dimmest notion of the length of time during which the position was maintained and that the whole experience was very hazy. (Further details will be given in connection with the discussion of anaesthesia.) Now if the *S* reacts to the manipulation of the arm mainly as a disturbance, it will be difficult to remember the incident because it is not essentially different from other disturbing factors such as accidental sounds and other details of

uncomfortable postural attitude; but if his curiosity is aroused, the incident will stand out more clearly. Usually both attitudes are present and the clearness of memory is a function of their relative strength. Again, the *S* who reports that his arm remained in position is ignorant of the fact that his own effort kept it there. The *S* who reports that he kept it there because he thought that that was what I wanted was not hypnotized at all, if his memory is trustworthy. But frequently that 'memory' is a secondary elaboration of his dim appreciation of the fact that his effort to coöperate resulted in the maintenance of the position; if he had stopped trying to sleep, the arm would have relaxed. This point will meet us again and will be supported by additional evidence.

Let us consider another phenomenon. The *S* is told that he cannot open his eyes. If an observable response does not occur, he is told to try. From the various responses which I have observed, I select the following. (1) Complete failure. The *S* opens his eyes and is wide awake. (2) The eyes open with difficulty. The eye-balls are turned upward and inward as in sleep. The eyes close again spontaneously and the *S* accepts other suggestions. After hypnosis he does not remember that he opened his eyes. (3) The *S* contracts strongly the muscles of the forehead and other facial muscles, but the eyelids are motionless. After hypnosis, he reports that he tried hard, but could not open his eyes. Observers whom I have called in to witness this, do not notice the passivity of the lids until their attention is called to it. The *S*s themselves never recall the fact, but they do report sometimes that their lids seemed to be made of iron or the like. (4) As above, but the eyelids flutter. Obviously the opposing muscles are strongly contracted. This fluttering of the eyelids is frequently observed in advance of any suggestion while the *S* is trying to sleep, and it seems obvious to me that the *S* finds it difficult to keep his eyes closed. In explanation it may be said that keeping the eyes closed is with most of us a part of the behavior-of-trying-to-sleep. The reader is reminded of the fact that most of the *S*s closed their eyes voluntarily when they began to try to sleep. If the *S* understood the situation completely, he might say, "I have tried hard to keep my promise to do nothing but try to sleep. Several times I have been on the point of succeeding, but each time you have disturbed me by raising your voice or changing the monotonous talk about sleep to which I had just become adjusted. If it had not been for my promise, I would have opened my eyes and shifted my position a number of times, but, with some effort, I have kept myself from that. And now, just as I have become so accustomed to keep-

ing my eyes closed in spite of these disturbances that I am doing it automatically, you are asking me, in effect, to keep on trying to keep my eyes closed and to try to open them. It's absurd." But inasmuch as the *S* does *not* have an adequate understanding of the situation, he endeavors to open his eyes and to keep on trying to sleep (*i.e.* to keep them closed) at one and the same time, and achieves one of the compromises described above, such as trying to open his eyes with his forehead and keeping them closed with his lids. We may say, subject to later modification, that the *S* cannot open his eyes because he is trying to sleep, but does not understand that the resistance he encounters *is* his effort to sleep.

It should be said also that the *S* is more or less drowsy. We meet here one of the difficulties encountered by a writer who tries to portray the simultaneity of concrete fact with words which must perforce follow one another. An examination of the activity of feeling drowsy is essential for an understanding of the activity of coöperating drowsily with the hypnotizer. The topic is worth detailed presentation and argumentation, but in the present article I must be brief and dogmatic. For an account of the physiology of the phenomena I refer the reader to the literature. We may note, however, that the physiology of the behavior (trying to sleep) which we have discussed so far is concerned mainly with postural and facial reflexes, whereas now we become involved in changes in circulation, respiration, and the like. But from our point of view we are concerned mainly with the realization that we are not concerned primarily with the analysis of the *experience* of drowsiness. My readers, I take it, are acquainted with the experience and I shall designate it merely as an organic experience *sui generis*. From our point of view we are concerned mainly with noting that drowsiness (like awareness) is a noun which designates an activity, viz. the activity of feeling drowsy—*and that this activity is different and distinct from the behavior of trying to sleep.*

Now it is a well-known fact that the drowsy state which precedes and follows sleep is characterized by great suggestibility. An individual in that condition will give verbal assent (sometimes more than verbal) to any proposition, no matter how absurd, provided the drowsy condition does not terminate. But if he objects, he is awake. He may promise to get up in 'just a moment,' but if he refuses, the hypnagogic state has come to an end. He may agree that the moon is made of green cheese, but if he objects to your nonsense, he is no longer very sleepy. Without going into the matter very deeply we may say that the drowsy individual perceives (hears) the language addressed to him mainly as a disturbance. The fact that the eyes

of the sleepy individual close (frequently without his knowledge) and remain closed is itself a protective activity which the individual (usually) does not understand at the time he is engaged in it. Now when the hypnotized subject cannot open his eyes and encounters resistance, that resistance is composed of two components. In the first place he feels, but does not understand, that the behavior of opening his eyes is incompatible with the behavior of trying to sleep. In the second place he feels, but does not understand, that he prefers to keep his eyes closed. The phenomenon is the resultant of the fusion of two different and distinct activities. It should be noted also that the activity of trying to sleep is social behavior inasmuch as it is the form which coöperation takes. It is a response to the hypnotizer. But the activity of feeling drowsy is not social in character. I shall call it organic behavior and this distinction will be developed as we proceed.

Now at the beginning of the induction of hypnosis the wide-awake *S* engages in the activity (described in detail above) which he '*calls*' trying to sleep. At that time I called it by the same name merely emphasizing that that is the form which coöperation takes. When the *S* begins to feel drowsy that activity integrates with the former activity. At the same time the *S* ceases to realize what he is doing. We must appreciate that realizing what one is doing is an activity which is not necessarily an integral part of what one is doing. Indeed, when the hypnotized *S* feels very drowsy his condition differs from ordinary sleep only in that, with the activity of sleeping, there is integrated the activity which, up to now, we called trying to sleep. If such an *S* is left to his own devices he will cease trying to sleep and will merely sleep (normally.) Accordingly, I wish to emphasize now that trying to sleep is trying to coöperate and that the drowsy *S* is not in a condition in which he is able to engage in the activity of making fine distinctions between different forms of coöperation. Accordingly when the hypnotized *S* tries to open his eyes and encounters resistance, I prefer to say that this resistance is his drowsy effort to coöperate, instead of saying that it is his effort to sleep. Coöperating drowsily is the behavior of the 'organism as a whole.' The organism is not engaged in two activities, but in a single one which is the resultant of the integration of two activities.

Let us consider another phenomenon. The drowsy, coöperative, but unanalytical *S* is told that his arm or hand is getting rigid. It does. I tell him that he cannot close the hand and, if that does not produce a visible response, I tell him to try. Let us suppose that the suggestion succeeds, *i.e.* that muscular contractions ensue which look like unsuccessful efforts to close

the hand. After hypnosis, some *S*s report that they could not close the hand, others that they seemed unable to try very hard, and still others say that they did not really try; they were not sure that I really wanted them to. With some of the latter I tried out a procedure of which the following case is typical. I pretended to be indignant with the individual because he had faked results, and assured him very emphatically that I did want him to try. At the same time I emphasized again that he must follow directions and must never stop trying to sleep until he had succeeded in falling asleep. The experiment was repeated and, so far as one could judge by appearances, the results were unchanged, *i.e.* the hand became rigid and the *S* seemed unable to close it. After hypnosis he reported that he was convinced now that he could not close his hand at the same time that he was trying to sleep, but he felt that he could stop trying to sleep at any time he chose. I proposed to him that he was to consider himself free to cease these efforts when I told him that he could not close his hand, but that he was to follow the old instructions up to that point, that is until after his hand had grown rigid. But when the experiment was repeated, the hand did not become rigid, although I used all the tricks in my repertory to bring about rigidity. Afterwards the *S* said that he did not seem to be able to give his entire attention to trying to sleep in the way he had done before. He said that from time to time he became afraid that I had succeeded in producing the same phenomena as before without his knowledge and that, consequently, his 'trying to sleep' was a broken and fitful performance. In other words, the *S* had understood from the beginning the facts which I am trying to put before you, but his understanding was extremely vague; as it became clearer, the phenomenon (of the rigid hand) disappeared. So much for the inability to close the hand. The maintenance of rigidity had become a part of the behavior of trying to sleep. As for the production of rigidity, if the respectful attention of a wide-awake *S* is secured, it will be found that he responds to the frequently repeated suggestion that his hand is getting rigid in a great variety of ways; subvocal, oculomotor, frequent changes in the tonus of the forearm, and the like. These, the so-called motor accompaniments of attention, are some of the ways in which a normal wide-awake *S* attends to the quasi anticipated phenomenon. In support I cite the following class experiment which I perform habitually. The class is told that the success of the experiment depends upon undivided attention and the members are warned that that will be somewhat difficult as the experiment is monotonous. Then I tell them for about 5 min. that their right hand is getting rigid. In a few

cases that actually happens. As soon as the experiment is apparently abandoned I ask how many feel that their right arm feels tired and as if it had been under considerable strain. Almost the entire class agrees invariably. In the case of the drowsy, coöperating, unanalytical *S*, the subvocal and, perhaps, the oculomotor responses tend to drop out, and the coöperative attitude of the *S* exerts a selective influence on the changes of tonus. Relaxing is *felt* (not thought) to be non-coöperative, so that tensing is the only method of attending to the quasi-expected phenomenon left to the *S*. Accordingly the hand becomes rigid. The behavior of making it rigid *is* the behavior of testing out the truth of the assertion of the hypnotizer. Besides we must remember that the *S* is drowsy and, obviously, the chief purpose of the responses which a drowsy individual makes to attempts at conversation is to keep the disturbance at a minimum. Tensing the hand is combined more easily with the behavior of trying to sleep than relaxing it. I should like to say also that I am not attempting to show that the acceptance of suggestions *necessarily* results from 'trying to sleep' and being drowsy. Suggestions sometimes fail. Success depends upon the details of the behavior of coöperating.

It should be interesting to compare this account of the phenomenon with one involving the idea of dissociation. I quote McDougall, "The patient will fail to achieve a forbidden movement—because, when he tries to make the movement and succeeds in innervating the proper muscles, the antagonistic muscles come into play and prevent the movement. At this stage, then, there is manifested a certain splitting of the personality, a conflict of one part against another: the muscles of one set obey the one part, the conscious willing subject; the antagonistic muscles obey some other part of the personality, which understands and is subservient to the commands and suggestions of the operator. This simple experimental evidence of division of the personality is of the first importance; for just such division is the very essence of many functional disorders."[3] My explanation of the phenomenon is that it is a quasi-mechanical aspect of the behavior of the subject and that the subject does not understand these quasi-mechanical aspects. If McDougall, instead of preaching that behavior is purposive, had attempted to understand the purpose or meaning of the specific behavior which he was observing, he would not need to speak of *muscles which obey the will of a dissociated personality*, nor would there have been any need of elaborate assumptions about the condition of the nervous system which *explains* dissociation.

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[3]William McDougall, *Outline of Abnormal Psychology*, 1926, 87.

As the hypnotizer continues working with the *S*, the latter becomes more and more adapted to the situation. His behavior is to be thought of less and less as 'trying to sleep' and more and more as maintaining a coöperative attitude in which drowsiness and respect for the operator are important factors. The point throughout is not only that the *S* maintains this attitude, but that he understands only in a very dim, vague way what he is doing. (In deep hypnosis I should say he does not understand it at all.) In such a situation the acceptance of a suggestion is as a rule the path of least resistance. If a great authority tells us what is what, the easiest thing to do is to agree—with mental reservations. The hypnotized *S* omits the mental reservations, because he is too sleepy, too lazy, too indifferent to make them; and, most important of all, if a mental reservation should begin to stir, the *S* finds that he is blocked by an unknown incomprehensible force, just as he does when he tries to open his eyes. That force is his feeling that he is not coöperating—but the *S* does not understand that. Accordingly, when you tell him that he is at the North Pole and ask him to describe the scenery, he begins to talk about polar bears.

In the last paragraph the critical reader will have noted that in preparing the ground for the explanation of a different kind of phenomenon, viz. hallucination, there occurs a shift of emphasis in the description of the behavior. In explaining phenomena such as the inability to move the rigid hand, I have emphasized the fact that the individual is trying to sleep. It was not necessary to assume that the individual is drowsy and more or less unaware of the fact that he is trying to sleep and that, consequently, the individual, who is not engaged in the activity of making fine discriminations, may be tricked into exceeding his original instructions in which coöperation was defined as 'trying to sleep.' As we proceed, we shall find that individuals differ enormously in the extent to which they may be tricked. It will be best to begin with an account of the cases where suggestion fails. Let us suppose that I have succeeded in suggesting rigidity of the hand and inability to open the eyes. Next I tell the *S* that he is at the North Pole and ask him to describe the scenery. The *S* does not reply. I repeat my request several times. The lips move, the facial muscles work, and the individual seems to be trying to speak, but utters no sound. Now if I keep insisting on a reply, the *S* is likely to open his eyes, declare that he saw nothing, and that he was not asleep anyway. At the same time he remembers about the inability to move his arm or open his eyes and is unable to account for these phenomena. If asked, "Why did your lips move without speaking?" he is likely to reply that he found it difficult to speak.

(Certainly it looks that way in the majority of cases.) He is unable to account for this difficulty. I account for the failure by assuming that the *S* was not sufficiently drowsy—or not sufficiently stupid, or not sufficiently friendly, or not sufficiently filled with mystic awe—to suppress a critical reaction. Although the critical reactions are not suppressed entirely, it seems obvious, however, that they encounter a resistance which the individual does not understand. In cases where the suggestion (of an hallucination) succeeds, I assume that the critical tendency is weaker and the resistance stronger. Consequently the suggestion has about the same effect as the request to imagine himself at the North Pole (which of course is what the *S* actually does). Indeed, after I have failed with the suggestion, "You are at the North Pole," I have sometimes resorted to the request "Imagine yourself (*e.g.*) in Kansas City" and have succeeded. In such cases the *S* frequently reports afterwards that the experience had reality and is not to be confused with wide-awake imaginations. In short, in order to produce an hypnotic hallucination the hypnotist must work to arouse the tendency to imagine the desired experience and to weaken or abolish critical reactions. As hypnosis is usually conducted, the operator will be aided enormously by the fact that critical reactions encounter a resistance which the *S* does not understand. When the hypnotist does not understand that either, he will of course adopt one of the current theories of hypnosis.

With reference to hallucinations and illusions, the question may be raised whether the *S*s really see the hallucinations and illusions. It may be that we are dealing merely with a verbal reaction. That is to say, the *S* may be describing the scenery at the North Pole without actually seeing it. The problem involved here is extremely difficult. Meantime some of the facts may be of interest. The suggestion of an hallucination sometimes fails. In such cases, as stated above, when the *S* is asked what he sees, he will, as a rule, not answer at all. If an answer is insisted on, he may cease to coöperate, open his eyes, and declare he saw nothing. In other cases he may continue to coöperate but say that he saw nothing. On the other hand, if an *S* describes an hallucinatory experience and if, after hypnosis, memory is restored, the *S* invariably reports that he saw what he described. Moreover, it is possible sometimes successfully to suggest hallucinations to *S*s who do not accept any other suggestions. In such cases amnesia does not occur and the experience is remembered as a dream-like vision. In the case of illusions, the facts are more complex. For example, the *S*, whose eyes are closed, is told to play with the kitten and is handed a handkerchief. Perhaps he strokes the handkerchief. If told

to put the kitten on the floor, he places the handkerchief on the floor. But after hypnosis he remembers clearly that he manipulated something that might have been a handkerchief and that was not a kitten. In such cases there is rarely an amnesia for the incident. In a few cases the *S*s say that they acted as they did because they thought I wanted them to, but in most cases they do not seem to have a clear conception why they acted as they did. They say it seemed to be 'all right,' or that there did not seem to be any reason why they should not, or the like. It seems to me that these are the cases where Janet's characterization of 'facile complacency' comes nearest to hitting the mark. Nevertheless if an *S* of this type is asked (during hypnosis) to describe the color and appearance of the kitten, the *S* will answer after a struggle, that he does not see a kitten, after the fashion of the incidents described above. Apparently there are limits to 'facile complacency' and it is not quite the same thing as deliberate simulation. On the other hand, if the *S* describes the color and appearance of the kitten during hypnosis, *it is an invariable fact* (in my experience) that he will declare after hypnosis that he really saw and felt it. As a rule, there is amnesia for an incident of the latter type and memory has to be restored by special methods. It seems to me that these *S*s are coöperating without having a clear idea where coöperation ends and simulation begins. My *S*s seem to draw the line at verbal simulation, but of course it does not follow that all *S*s do so. I incline to the belief that the testimony of my *S*s is entitled to the same credence as regards the visual character of their experiences as if they were reporting hypnagogic hallucinations.

In the case of color illusions the facts are somewhat similar. *S* is told, for example, that he will be shown a blue book. Instead he is shown a red book and asked what color it is. The suggestion may fail; but I have never had an *S* who said, in hypnosis, that the book was blue, tell me, after hypnosis, that it was red. As a rule, there is amnesia and, after memory is restored, some *S*s remember clearly that it was blue. Others say that it was not a very good blue, but looked kind of blue. One *S*, on being shown the book, tried to speak but was unable to do so for several seconds. Finally he said, "It's blue—blue." The tone of voice indicated (to me) relief and an effort to contradict any doubt of the blueness. After hypnosis there was complete amnesia, but when memory was restored he said that the book looked red at first and then turned blue. It seems to me that we have here two possibilities and no more. Either the *S* actually succeeded in seeing a blue book, or else he continued to see a red book but reacted to the total environment not only

by telling me that it was blue but succeeded in convincing himself (as indicated by the tone of voice). The first alternative is perfectly possible in spite of the fact that its physiology is obscure. About all we can say is that the behavior of seeing a blue book (including the physiology of that behavior) occurred as a response to the total environment. I might add that the physiology of a 'subconscious' perception of a red book is equally obscure. The second alternative is possible also and opens up the possibility of 'tapping the subconscious' by means of automatic writing. In my opinion both alternatives occur actually and I hope to be able to test this opinion by means of experiment in the near future.

In the case of negative hallucinations we have, fortunately, a more complex problem. For example, *S* is told that Mr. X has left the room; that he will be unable to see Mr. X. Then he is told to count a group of three individuals one of whom is Mr. X. He counts two and ignores the presence of Mr. X in every possible way. When memory is restored, after hypnosis, he recalls that the group looked peculiar, but is unable to characterize that peculiarity more closely. It has been pointed out frequently that the negative hallucination implies some sort of positive reaction to the stimulus; somehow, and in some sense, the individual knows what not to see. The problem here is very similar to the one which confronted us in the case of positive hallucinations and illusions. Does *S* succeed in convincing himself that he does not see X in spite of the fact that he does see him, or does he react to an environment containing X by seeing *a peculiar group?* In this case, however, further discussion may be more profitable because, whereas we know practically nothing about the concrete details of the simple behavior of seeing blue, we are more fortunately situated with reference to the complex behavior of seeing an acquaintance. Normally there occurs at such times the beginning of the act of greeting the friend. The head and body begin to orient themselves toward the stimulus, there is the incipient handshake or wave of the hand, the beginning of a smile, the initiation of a verbal greeting, the beginning of being pleased (visceral components), and the like. These various activities, probably, do not arise with absolute simultaneity, but are initiated in rapid succession. We do not perceive these activities, but we perceive what these activities mean. We do not *feel* that we have begun to speak, but we *see* a to-be-spoken-to-individual. Furthermore, so long as that sort of activities is maintained, so long do we see that sort of an individual. The question is, did such activities occur in the situation in which *S* reported as having seen a peculiar group? In reply, I have no desire to

be dogmatic. Doubtless the details vary from individual to individual, but I maintain that the following account is essentially correct and does actually occur at times. Some of the salutatory activities just mentioned become initiated and, if they developed and if that were all that occurred, they develop into the behavior of perceiving Mr. X. But, just as the meaning of his activities is beginning to dawn, just as he is beginning to perceive Mr. X, an avoiding reaction supervenes. *S* does not wish to see what he has begun to see. The details of the avoiding reaction might be the incipient behavior of closing the eyes, turning away, and the like. The salutatory reaction is suppressed. It is not repressed into the subconscious. It is annihilated. It does not occur. Mr. X is not perceived. The avoiding reaction is maintained throughout. Consequently, what the individual perceives is a situation with a problematic aspect. The other members of the group are seen clearly enough to be counted, but the group as a whole is *peculiar*. Furthermore, this peculiarity is not nearly as interesting to *S* as it is to us. If it were, the negative hallucination would not occur. The peculiarity is satisfactory and satisfying. It is perceived as a not-to-be-inquired-into-aspect of the situation. It seems to me that such an account of the negative hallucination is more satisfactory and more intelligible than the facile assumption of unconscious or subconscious perception.

We are confronted with similar problems in the case of cutaneous anaesthesia and analgesia. The *S*s act and talk as if they did not feel contact or pain. If acting in that way is taken to be at least a part of the behavior of feeling pain, it becomes obvious that the response to the environment becomes profoundly modified in hypnosis. The question remains whether any part of the behavior of feeling contact or pain persists. In the case of contact we have the evidence furnished by automatic writing and through the report of so-called co-conscious personalities that it does, at least sometimes. But in case of stimuli which would ordinarily result in severe pain I am not familiar with reports of the subconscious or co-conscious persistence of pain. On the contrary, it is stated in the literature that even the so-called physiological indices of pain, such as the dilation of the pupil, are absent in cases of profound analgesia.[4] Accordingly, we may conclude that the behavior of feeling pain does not occur. The question remains, what sort of a response *is* made to the environmental stimulation. In order to sharpen the problem, I shall define pain as an unsuccessful avoiding reaction. Now then, if the hypnotized individual does not attempt

[4]It is well known that the dilation occurs also in fear. It seems to me that the absence of fear in hypnosis has not been considered by previous authors.

to avoid or modify the lacerated condition of tissue, for example, *what does he do?* Surely, some sort of 'physiological' processes are initiated by the laceration. Can we succeed in discovering the meaning of that behavior? I think that my experience with the induction of these phenomena furnishes us at least a hint. At the beginning of my experiments I had little difficulty in inducing all of the phenomena described so far, but I had no success whatever in the induction of anaesthesia and analgesia. This fortunate incident, which was due probably to the character of my *S*s, had the effect of directing my attention to the search for favorable conditions. To be brief, I found, if the arm of the *S* was placed in a horizontal position and left in that position for about 15 min., that anaesthesia and analgesia would supervene spontaneously, without suggestion from me. Not only did the arm of the *S*s seem to be insensitive to contact, pinches, and pin pricks, but the *S*s declared themsleves ignorant of the position and even of the very existence of the arm (absence of localizing activity). Of course, the failure to feel the painful fatigue sensations which ordinarily supervene is itself an analgesia. Now, at the beginning of our discussion of hypnosis, we accounted for the maintenance of the position of the arm by saying that it was a part of the behavior of coöperating by trying to sleep (maintenance of relative immobility). Distractions of all sorts which would result in movement under other circumstances result in increased tension instead. In the same way, instead of the behavior of feeling pain, we have an increase of coöperative tension. This account of hypnotic analgesia agrees with the widely known facts of emotional analgesia when, *e.g.* during the heat of battle, an injury which would ordinarily be painful is not felt at all. The response to the injury is a reenforcement of the on-going activity. The behavior of feeling pain does not occur. There is no pain.

We come now to the problem of amnesia. Before beginning its discussion it will be necessary to enlarge upon the topics of social behavior and self-conscious behavior touched upon briefly above. In *Behavior and Conscious Behavior*, I defined the social act as an unfinished act which meant its own completion, and as an act which, as I put it at that time, had a double meaning or reference.[5] In the *Problem of Meaning in Behaviorism*, I added that all unfinished acts, non-social as well as social, meant their own completion.[6] That left as the diferentia of the social act, its double meaning. I knew of course that the activity of picking up a hammer to drive a nail refers directly to the hammer and indirectly to the nail, just as the social act of asking another to open the window refers directly

[5] *Op. cit.*, 210, 212. [6] *Op. cit.*, 242.

to the other and indirectly to the window, but at that time I was not prepared to meet the logical difficulty and social behavior was left without a well-defined differentia. In order to meet the difficulty I shall have to remind the reader that we are dealing with the behavior of the organism as a whole. Considered in that way, the social activity of the organism is never purely social; it has a social aspect. I shall state this position first in a series of formal definitions and shall then develop it into a brief genetic account. In meaningful behavior the organism means to modify an experienced situation. If and so far as the behavior is social, the organism means to modify an experienced situation in which it perceives the meaningful behavior of another to be a factor. (The organism need not perceive correctly.) If and so far as the behavior is self-conscious, the organism means to modify an experienced situation in which it perceives its own meaningful behavior to be a factor. In other words, social and self-conscious behavior are not different kinds but different aspects of meaningful behavior. These aspects are not merely logical; they are psychological—or else logic is without psychological foundations, and psychology devoid of logic.

Racially and ontogenetically regarded, social and self-conscious behavior have developed as aspects of organic and presocial activity. The infant perceives neither social nor nonsocial objects because it does not perceive objects at all. The problem of describing what such behavior means, is perhaps the most obscure problem which confronts the psychologist. There is, for example, the famous solution of William James who said that the world of an infant must be a 'great, big, buzzing, blooming confusion.' Meantime the infant does not show the slightest indication of being confused. One might say that the meaning of infantile behavior is vague, but it does not seem to be possible to believe that the infant experiences a vaguely significant situation because that would seem to presuppose a contrast with experiences the significance of which is clearer. It seems to me that what infantile behavior means is neither vague nor confused, but is described best by the term *undeveloped*. This undeveloped character is represented best by very general terms such as pleasant and unpleasant, or satisfying and non-satisfying. Withdrawal and approach seem to me to be accidental consequences of organic infantile behavior; I cannot believe that the very young infant means to withdraw or approach. That is a later development as localizing behavior patterns become organized somewhat definitely. I would define the activity of experiencing satisfying experience as behavior which *means* to attain more of that kind of experience

and the activity of experiencing dissatisfying experience as behavior which *means* to attain less of it. These very general terms imply, of course, a great deal of internal variety and variability. On account of the undeveloped character of the behavior, it is possible, however, to describe this internal variety only by means of reference to stages of development which have not as yet been attained. Thus the infant responds to an environmental circumstance such as a loud sound, not by hearing a sound, but by feeling distress. Out of this matrix the behavior of hearing sound develops. We may gain some acquaintance with the concrete nature of such experiences by recalling instances when we were on the verge of 'losing consciousness.' When we are very sleepy, when we are sick and dizzy, when we are suffering intolerable pain, the highly organized world fades out and is replaced by an undeveloped world of organic experience. At such times the organism reverts to infantile modes of behavior. In Freudian terms, we regress to infantile or even to intra-uterine experience. At any rate, out of such a matrix of organic activity there develops the behavior of perceiving *things or objects* more or less clearly demarcated from the organismic background. Demarcation develops as far as it does develop in any given instance. It is seen, heard, felt, or briefly, it is perceived as clearly as it is perceived. I am interested but slightly in the abstract statement that an absolutely perfect demarcation is unattainable. Presumably most people will regard the maximal clearness which they have achieved as perfection. But such perfection *is* an achievement and is preceded, developmentally, by lesser degrees of perfection.

As the activity of perceiving *things* is developing, it develops of course in a variety of ways. That is, we perceive different kinds of things. First of all the infant cries at situations. Later the cry comes to be focalized upon things or objects. Very gradually we learn not to cry at some objects and that negative form of learning continues through life. To be brief, we learn to react to human beings and animals in ways which differ typically from our reactions to sticks and stones. That is, we acquire social and non-social behavior. The detail and the degree of this differentiation varies with the organism, the stage of development, and with circumstances.[7] Accordingly it is impossible to define social and non-social behavior by complete enumeration. The attempt could result only in an arbitrary revision of the dictionary. The genetic transition from pre-social to social and non-social behavior is gradual and there is no exact point at which one ends and the other begins. In other

[7]After things have begun to emerge from the organismic background and before differentiation between different kinds of things has progressed very far, it might be advantageous to distinguish a pre-social stage.

words, considering the wide-awake human adult, the behavior of the organism-as-a-whole is never merely social or merely self-conscious, but in stating what such behavior means we are able to distinguish organic, pre-social, social, non-social, and self-conscious aspects.

I acknowledge freely that it is not always possible to discover what a given form of behavior means. I acknowledge that it is not always easy to determine whether a given aspect of behavior is social; but, inasmuch as such forms of behavior are not utilized in my account of hypnosis, these admissions do not weaken that account. In this article the term social is applied only to activities about the social nature of which there can be no reasonable doubt. Coöperation is social, and the fact that the meaning of the coöperative behavior is undeveloped (vague) is the fault of the facts and not of the description of the facts.

We may pause a moment for the double purpose of illustrating social, pre-social, and organic behavior and of reading these terms back into the account of hypnosis given so far. Thus, in the case of individuals who maintained a horizontal position of an arm for a long time we noted that the arm became anaesthetic, and that the individual lost all sense of the position and existence of the arm. We urged that the response to the 'fatigued' condition of the muscles is a reënforcement of the coöperative activity. That aspect of the behavior is social. But, just as the organic 'experiencing' of the infant has internal variety, so the social behavior of the hypnotized individual has internal variety which varies with the condition of the organism. Just as the behavior of hearing sound is undeveloped in the case of the infant, so the activity of feeling fatigue in the arm is undeveloped. The individual regresses to an infantile mode of behavior. Moreover that form of organic behavior is not particularly abnormal. The internal variety of much of our behavior is described best by saying that we feel tense or relaxed, comfortable or uncomfortable, or the like. Further development of such forms of behavior takes place comparatively rarely. Again, in the case of the negative hallucination described above, the *S* responded to a human being by perceiving him as a peculiar aspect of a social situation. Clearly, that is an organic aspect of social behavior. If he had perceived him as a chair (and such complications happen sometimes), the behavior would have been non-social. Or the undeveloped organic aspect of the activity, which may be described as the perceiving of the peculiarity of the situation, may develop into a non-social aspect; that is, an aspect in which the experience of an object but not of the meaningful behavior and attitude of that object occurs.

We may return now to the problem of amnesia. The facts of amnesia are the stronghold of the theory of dissociation. At best, this theory is not an explanatory theory. As an account of a contracture or paralysis it does not possess even descriptive value. But when a large number of real and imaginary experiences cannot be remembered in the normal condition and can be remembered in hypnosis, the term dissociation seems to have considerable descriptive force. Explanation, however, must

consist in making the dissociating process intelligible. It is this problem which we shall attack.

Post-hypnotic amnesia is a matter of degree, varying from almost normal memory to so-called complete amnesia. Moreover, it is usually possible to restore 'lost' memories in one way or another. Accordingly we must explain why the subject remembers what he does remember, why he forgets what he does forget, and the technique of restoring memory. Now, fortunately, 'normal' remembering and forgetting are not so very different from the hypnotic variety. For example, I have read aloud to my sick father without having the faintest notion of what I was reading. At such times I am quite unable to 'remember' what I have read for the reason just indicated, viz. that I never knew. When we are reading a book, the activity is of course our own, but we do not perceive our own activity. What we perceive is very much like seeing (and hearing) the speech of another. Our reaction to what we see (*i.e.* to our own activity) is very much like our reaction to the speech of another. We are interested or indifferent, amused or bored, and the like. Our memories of what we have read are very much like our memories of what we have heard. We remember what we reacted to. We remember that the speech was interesting or the reverse, we remember a jest, a happy phrase, and the like. In reading to my father, I remember raising and dropping my voice in accordance with the punctuation, noting my failures, attending more closely to the business of reading adequately. I do not remember, however, the thought of the passages, because in the social situation which I created by reading the passages and reacting to them there was no thought—of the kind ordinarily referred to when we speak of the thought of a printed paragraph.

It makes little difference whether we call reading a form of self-conscious behavior. The essential thing is that a social situation is created through the activities of a single organism. Even in instances where the thoughts which I think are clearly 'my' thoughts, I cannot find that all of my thoughts are of equal interest and importance—to me. However important such material may be to the psychoanalyst, however important it may seem to me when I am in a psychoanalytic frame of mind, there are times when my thoughts seem very unimportant and are forgotten promptly.

Much of so-called post-hypnotic amnesia is of this character. Some of the facts are not forgotten at all; they were not known in the first place. Other facts were known, but seemed very uninteresting and unimportant and are forgotten on that account. Thus the individual does not forget that his arm was

in an horizontal position for a long time. He never knew it, any more than I knew a moment ago that I was sitting cross-legged. The 'horizontal position' and the 'cross-leggedness' were perceived as undeveloped organismic aspects, not as separate facts. In the same way, in the case where the individual cannot close his rigid hand, if he perceives nothing but a slightly disagreeable tension vaguely localized, he will have difficulty in remembering that, but if this tension develops into the perception of an interesting fact, that is more likely to be remembered. Considerations of this sort have some force even in accounting for the forgetting of suggested hallucinations. Thus if the *S* is told that he is at the north pole and asked to describe what he sees, he may display very little spontaneity. He may say that he sees snow and ice, and stop. Asked for further details he gives one or two every time he is prodded. He does not deplore the poverty of his imagination nor does he try to make up for his lack of spontaneity by increased effort. Even in cases where the hallucination is more spontaneous and richer in detail, it is, as I think, a legitimate assumption that what the *S* talks about and, perhaps, visualizes is not nearly as interesting to him as it is to the spectators and to the hypnotizer.

Ignorance and lack of interest, however, do not suffice as an explanation of amnesia—not without further elaboration. In the first place these uninteresting memories can be restored. In cases where the hypnotized *S* engages in animated discussion, or when an exciting hallucination is suggested, the hypothesis of lack of interest may seem inadequate. The reader will recall that the behavior of coöperating is the maintenance of an attitude. That attitude is neither physical nor mental; it is a conscious, meaningful attitude. It is a way of experiencing experiences. Just as the enemy-to-be-run-away-from *looks* different from the enemy-to-be-attacked, so the hypnotic experience is different from the non-hypnotic variety. It is a commonplace that experience has a focus and a fringe—a figure and a background. In ordinary experience visual and auditory aspects are prominent in the background, and organic and kinaesthetic aspects are less prominent.[8] But when our attention becomes riveted tensely upon the person of an enemy in anger or in fear, the background becomes less visual and more organismic—we experience a tense situation.[9] In hypnotic experience also visual and auditory aspects are, I believe, less prominent in the background and *tension* is more prominent. That is, every hypnotic situation has a peculiar, characteristic aspect—it is a

[8]The presence of this organic and kinaesthetic aspect is vouched for by the 'pressury' part of the 'pressury gray' of our 'trained introspectionists.'

[9]The shrinking of peripheral vision in hysteria can be connected with facts of this character.

tense situation. Now I have not become converted to the position that meaning is kinaesthesis. I maintain that experiencing-a-tense-situation is a unitary act. Tension is not always coöperation, nor is coöperation always tension, but tension is the form which coöperation takes in hypnosis. In other words, the *S* responds to the suggestion of the hypnotizer by experiencing an hallucination—for the hypnotizer. Now, when amnesia supervenes, it is a fact that 'memory can be restored' by means of a partial restoration of the coöperative behavior. I conclude on the basis of this evidence that this characteristic, tense, coöperative aspect is the supremely interesting aspect of hypnotic experience. The *S* is not interested in the uninteresting fact that he is talking about the north pole (and, perhaps, visualizing it) because he is too intensely interested in visualizing and believing it; and, vice versa, when amnesia is more or less incomplete, I conclude upon the basis of the evidence that the *S* was more or less interested in the more or less interesting fact that he experienced an unusual experience under unusual circumstances.

Once more. When I was a boy I had numerous fights and while fighting I saw the enemy clearly and I did not see much else. These things are easy to remember because at the time of the occurrences I had adequate insight into the relevance of what I saw. I was engaged in a meaningful activity which had not been completed and the meaningful behavior of the other boy was supremely interesting. After the activity had been completed, I had adequate insight into what I had done and its relation to the rest of my social life. But when the hypnotized *S* hallucinates, his insight into the relevance of what he sees is practically nil, because he does not understand what he means to do (*i.e.* that he means to coöperate by trying to sleep) and is doing it too tensely to inquire into the relevance of what he sees. The *S* is told, "You are at the north pole." His critical reactions, if any, are suppressed. He looks, *i.e.* he visualizes (or believes); but he is too interested in visualizing (or believing) to inquire into the relevance of what he sees (or believes). In order to remember, he must first be given an opportunity to become interested in what he has seen or believed.

The amnesic *S* who is asked to recollect what happened while he was asleep is asked to look for he knows not what. He does not know what to look for and he does not know how to look. If you are asked to remember what your experiences were on August 2, 1923, you have something to start with. The amnesic *S* has nothing. If, however, we reinstate the activity which developed into the behavior of talking about the north pole

in the first place, the repetition of that behavior is greatly facilitated—but mere repetition is not memory. The *S* who repeats his former performance exactly does not remember; he is re-hypnotized. In order to make it possible for the *S* to react to his own behavior in a way in which he did not react to it in the first place, we must avoid making the repetition too complete.

Now for our evidence. Bernheim has demonstrated in cases of apparently complete amnesia, that it is possible to restore memory by browbeating and cajoling the *S* until he does remember. I have repeated these experiments with similar results. But, *in cases of apparently complete amnesia*, I have found also that when these *S*s begin to remember, they are practically as suggestible as they were in hypnosis. Tell them that their hand is getting rigid—it does. Tell them they are asleep and they are re-hypnotized. These same *S*s may require several minutes of tedious preparation to hypnotize them the following day. While I am cajoling and browbeating, these *S*s try earnestly to do what I want them to do. They are coöperating. Now after cajoling and browbeating have been continued for 15 or 20 min., it begins to become somewhat monotonous. After all, the method has some points of similarity with other methods of inducing hypnosis. Partly on account of this similarity, partly by accident, coöperation takes the form which it took during hypnosis.

As additonal evidence I select the following cases. *S* is hypnotized, his left hand is made rigid, and he describes in considerable detail two hallucinations which are suggested to him. Next he is told that his left hand will become rigid after he has awakened when he is asked, "What is your name?" but that he will take no notice of the fact. He is awakened and asked for an account of what he remembers. Amnesia is almost complete. After considerable urging he remembers that there was something about the law building and something about a movie, but that is all. He is asked, "What is your name?" He replies and his left hand becomes rigid. Asked again to try to remember what happened during hypnosis, he describes the two hallucinations in almost the same words which he had used during hypnosis. *S* says, regarding these replies, that it is as if he were reading them out of a book. That is, a partial restoration of the attitude which he maintained while telling me about the hallucinations during hypnosis enables him not only to tell me about them again, but also to listen to himself and to understand what he himself is saying. That the partially restored attitude is coöperative is easily demonstrated. As soon as memory has been established, the *S* is told that his eyes are

closing and that he is asleep again. The eyes close and the *S* accepts two other suggestions. He is told to wake up and to remember everything. He does so, and afterwards the two hallucinations are remembered normally.

To cite another case, one *S*'s post-hypnotic memories are vague, dreamlike, and very incomplete. He had been given the post-hypnotic suggestion that he was to answer 'Jack Dempsey' to the question 'Who is your roommate?' The suggestion failed and, in spite of considerable urging, the *S* did not remember having received any instructions. The sleep method induction had been used. In order to restore memory, I instructed the *S* to stare fixedly into my eye and, when he was beginning to get tense, I asked quietly, 'Who is your roommate?' He began to answer "Jack—," stopped, laughed, and remembered the whole incident completely and clearly. In this case, the *S* 'remembered' his instructions—that is, while he was maintaining an attitude similar to the one he had been maintaining when the original instructions were given, he began to reply in accordance with these instructions, and then, reacting critically to his own behavior, he grasped the meaning of the situation in which the response began to occur.

The way *S*s do and do not remember about their behavior during the execution of a post-hypnotic suggestion is very instructive. In some cases they re-enter the hypnotic state spontaneously, must be reawakened, and exhibit complete amnesia afterwards. Less extreme cases which exhibit a distinct tendency in this direction vary enormously in detail. But the following case is typical. The post-hypnotic suggestion had been that the subject was to answer "Denver" to the question, "Where are you going this afternoon?" The suggestion succeeded and the *S* did not seem to know that he had said anything odd or absurd. To the question, "What train are you going to take?" he replied, "The Santa Fe." The question, "What are you going to do after you get there?" seemed to puzzle him a little, but after a few moments he replied, "Hunt for a job."[10] I have observed a number of similar cases. As a rule hypnosis can be reëstablished simply by telling the *S* that he is asleep. But if, after execution of the post-hypnotic suggestion, the *S* is engaged in ordinary conversation, he will, as a rule, return to his normal condition. If, after normalcy is judged to have returned, he is asked about the trip to Denver, he may or may not remember the absurd statements he has made. But, if he remembers, he does not as a rule become aware of their absurdity until after he has remembered them. He

[10]The reader will note that these replies are congruous with the original suggestion. The *S* is coöperating.

may say that he does not know why he said what he did, or that something made him say it, or that he did it to please me. These are some of the attitudes *S*s take toward their own behavior after it has occurred. Perhaps they are elaborations of the attitudes taken originally in hypnosis. But if the original behavior was addressed to me, and if the *S* did not take any kind of an attitude toward his own behavior, it becomes difficult for him to remember what he has never known.

When an individual in the normal condition points his finger at an object it is not necesssary for that pointing activity to be that object. Moreover, such an activity is genetically and typically a social activity—the individual points out the object to another. When an individual talks about an experience it is not necessary for the talking activity to be that experience, nor need he be reëxperiencing the experience at the time he is talking about it. There is no need of a 'mental' representative of the experience; the speaking organism *re-presents* it to another and, usually, to himself. The speech activity, with or without 'visual' or 'auditory' components, *is* the re-presentation. However displeasing these facts may be, however unscientific it may be to mention them, the facts themselves can be verified by the observation of any one who has not been trained too thoroughly in a method based on antiquated and discarded metaphysics. (And, somehow, I cannot convince myself that it is the essence of scientific method to ignore observable fact.) Now in post-hypnotic amnesia the amnesic individual cannot re-present a former experience to another nor to himself. Originally the activity of experiencing that experience was an integral component of the behavior of coöperating with the hypnotizer in the peculiar, characteristic fashion described above. On the positive side, it is possible to enable the subject to re-present the experience by reinstating, in part, the peculiar, characteristic activity which made it possible for the *S* to *present* the experience to the hypnotizer in the first place. On the negative side, re-presentation in any other way is difficult or impossible because the activity of relating the experience to other experiences did not occur in the first place. Concretely, in the north pole hallucination, in spite of the fact that the hypnotized individual was re-presenting a scene which he saw at a moving picture theater (as brought out by subsequent investigation), the individual did not remember having seen the 'movie,' nor did the north pole experience seem strange or familiar; it just was. Similarly, the fact that he was talking about the north pole, and possibly seeing it, did not seem strange nor was it related to former experiences in any other way; when it is remembered, it is remembered as having happened—that is

all. 'Dissociated hypnotic experiences' dwell in the same limbo as last winter's snows. But the activity of re-presenting these experiences is difficult, because it is connected in a peculiar, difficult, and unusual fashion with the person of the hypnotizer.

Summary

By way of summary let me repeat that the phenomena of hypnosis are resultants of the meaningful behavior of the individual in ways which that individual does not understand. I have not dismissed this lack of understanding as a brute fact, but I have studied it. Upon study it has resolved itself into an undeveloped aspect of meaningful activity. The meaning characteristic of all forms of hypnotic behavior is meaning to coöperate with the hypnotizer. This article consists of an exposition of the concrete detail without which these general statements are vague and useless. In view of that it seems to me that additional summarizing can do no good. Some readers will find it possible to misunderstand the article without artificial aid.

These general principles have the widest possible application. In politics, religion, business, and in 'mental' disease, in all walks and aspects of human life, the undeveloped aspects of meaningful behavior are profoundly significant. The thing to do is to go to the facts and work out the details. If we do, it will become superfluous to study the attempts of Watsonians to 'behave' as if they were scientists; and we may join Freud in his efforts to translate his epoch-making intuitions into intelligible language.

When this article was begun, it was my intention to write on hysteria also and that is the reason why the article was not published two years ago. The account of hysteria which I have worked out thus far is analogous to the present account of hypnosis. The phenomena of hysteria are resultants of the meaningful behavior of the individual in ways which the individual does not understand. In hysteria the important behavior is the activity of emoting. In the face of the protean character of hysteria I have, however, found the working out of the details so difficult a task that I have decided to publish this study of hypnosis which is, in a sense, complete.

Finally, it seems to me that many of the facts reported in the literature need additional detail in order to be interpreted. When Braid reports that a person who could not lift 10 lbs. in his normal state could swing 50 lbs. around his head with one finger in hypnosis, one wonders what the 'normal' state was like. Obviously, many of these cases were hysterical, though the authors neglected to mention that, if they knew it. Again,

the relation of physician and patient is a peculiar one and there is no limit to the complications which may arise. Janet has shown that Charcot's subjects were, virtually, trained performers. When hypnotic anaesthesia is used as an aid to surgery, the coöperative motive is quite unlike that of a student who volunteers to be a subject. Hope, fear, love, jealousy, and hate, are powerful *motifs*. Sometimes an author hints delicately at facts which might lead us to suspect that they might have played a part. Sometimes he says nothing. In my own work I have taken note of the emotion of hope which animates the soul of the student who means to 'work' the instructor for a grade; and I have noted other minor emotions. My account, however, attempts to explain only what I have observed, not what I have read.

PSYCHOLOGICAL EFFECTS OF FASTING

By JOHN ARTHUR GLAZE, Colorado College

THE EFFECTS OF PROLONGED FASTING

Problem. It was our purpose in the experiments here reported to study the psychological effects of relatively prolonged fasting. Three healthy adults, two men and one woman, went without food for from 10 to 33 days. Tests of several varieties were made at regular intervals before, during, and following the fasting periods.[1]

Earlier work on prolonged fasting.[2] One of the well-known fasts of some length was that of Succi, reported by Luigi Luciani.[3] During his 30-day fast Succi was observed only casually for effects upon mental processes. Other results, however, show that he made decisive improvement in tests of strength. His firm belief in the efficacy of fasting may have affected the results. Luciani doubted Succi's sanity and had little faith in other than the purely physical results.

One of the recent experiments on fasting was reported by Marsh.[4] This experimenter and his wife fasted for one week. The week preceding the fast they gradually reduced the amount of food taken, and the week following reversed this procedure. Short tests were used. The data secured during the control period were discarded for some unexplained reason. This period covered six days of testing, and should have been most significant for purposes of comparison.

The results secured in this experiment were not very definite. The experimenter thought there was some improvement in mental clearness and accuracy for both subjects. His conclusions about sex-differences have no value in view of the fact that only two subjects were used in the experiment.

The outstanding psychological experiment upon a long fast is that performed by Langfeld.[5] The subject was Agostino Levanzin, a lawyer from Malta, who believed a fast markedly increases mental acuity. He claimed to have pleaded a case in the courts of law after having fasted for 26 days.

Levanzin came to this country for the express purpose of demonstrating the beneficial effects of fasting upon mental processes. He entered the Boston Nutrition Laboratory of the Carnegie Institute of Washington and underwent a 31-day fast there. Eight tests were given daily during the experiment, all comparatively short ones. Unfortunately, the experimenter had no opportunity to run a long series of preliminary tests. The follow-up tests had to be discontinued because the subject withdrew from the laboratory after developing a colic. The ailment was attributed to the nature of the food taken immediately after concluding the fast.

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[1]This study was carried out in the Psychological Laboratory of the University of Chicago under the direction of Professor Edward S. Robinson.

[2]Experiments on short fasts are discussed at the end of this paper.

[3]Luigi Luciani, *Das Hungern*, tr. by M. O. Fraenkel, 1890.

[4]H. D. Marsh, Individual and sex differences brought out by fasting, *Psychol. Rev.*, 23, 1916, 437-445.

[5]H. S. Langfeld, On the psycho-physiology of a prolonged fast, *Psychol. Monog.*, 16, 1914, No. 71.

A year after the fast it was possible to test Levanzin again, but of course the time-interval involved was too great for the tests to throw any light on the process of recovery.

Levanzin showed a loss in muscular strength during the fast. Strength, as tested by the hand dynamometer, fluctuated from day to day, but showed a gradual falling off as the fast progressed. There was a gain in those tests involving the 'central processes'—by this phrase are meant tests of attention, perception, and association. Association and rote memory tests were also used, and several others. There was a decided gain in visual acuity as the fast progressed. At the end of the fast, Levanzin could detect the letter E, the largest one of the Snellen test card, in its correct position from twice the original distance.

The effects of practice in this experiment will never be clearly known. That there were practice effects is not denied. The most decisive facts brought out by the experiment were that muscular strength showed a falling off, and that the ability to improve mental operations through practice was not entirely arrested by the fast.

The lack of preliminary testing and a 'follow-up,' continuing for several days after a fast, has been a weakness of all previous experiments.[6]

Subjects. Three people were used as *Ss* in our experiment. They will be designated *A*, *B*, and *C*.

A is the writer's wife. She is a college graduate, and was engaged in the domestic duties of her household during the experiment. She did not prepare food for the family during the first few days of the fast, but found no difficulty in doing so after that. She fasted for 10 days.

A had undergone short fasts of a few days' duration several years previous to the present experiment, while attending college. She believed short fasts benefited her health, but she was not sure that they helped her mentally. She had never gone over 4 days without food before. Her previous fasts had been endured to get rid of very painful attacks of neuralgia.

She began the practice period on April 8, 1925, and was tested daily throughout the experiment. On May 18, 40 days after the first test, she began the fast. Testing continued during the fast as before. On the fifth day of the fast she developed a severe headache which continued for two days. She was in perfect health, as shown by physical and metabolic tests, preceding the fast. This was the only ill-health she experienced during the experiment. On May 27, after taking the tests, she concluded the fast. For 23 days after the fast, *i.e.* until June 19, testing continued. The entire testing period was 77 days in length. Further details are given below.

B, the writer, was a graduate student at the time of the experiment. He was pronounced in good health by an examining physician. Besides giving 5 hours daily, on an average, to the experiment, he carried two majors in the University—a course in experimental psychology and an advanced course in neurology.

B had never fasted before. He had no definite opinion about its mental and physical effects. He had read many claims about its rejuvenating effects and was interested in finding out what the results would be.

He began the practice period on April 25, 1925, and continued it for 32 days, or until May 27, the beginning of the fast. The fast was prolonged for 17 days, up to June 13. Testing was carried on daily throughout the entire experiment. The post-fast testing continued for 33 days, until and including July 15. The entire testing period was 82 days in length.

[6]Langfeld, himself, realized this deficiency in connection with his own study, but he was faced with circumstances which he was powerless to correct.

The health of *B* was good during the fast except for a slight headache on the tenth day. A physical examination on the fifteenth day of his fast showed him to be in excellent condition, except, of course, for the loss of flesh.

C is a man about 35 years of age. (All *Ss* were approximately the same age.) He had attained a secondary school education and is a lay scientist. He had fasted over a total of 300 days, previous to the present experiment, often in the interests of observing physiological effects. He had never taken psychological tests during a fast. He fasted 33 days for the present experiment. The longest fast that *C* had endured heretofore was one for 26 days. He had not undergone any fasts of over a few days length for several months previous to this experiment.

C believed that a fast improves one's health but doubted whether the results are worth the effort and inconvenience. He was quite certain that the "rational element," as he expressed it, is neither helped nor harmed by a long fast. He lived several miles from the psychological laboratory, and had to go back and forth from his home to take the tests. Fortunately, at this time, he was not employed in any occupation, and consequently could give considerable time to the experiment.

C began the practice period April 13, and was tested approximately every other day during the 110 days of the experiment. In only two instances were testing days less than a day apart. The greatest interval was two days, and it occurred several times. *C* took the tests 48 days out of the 110.

He was pronounced in good health at the beginning of the fast by a physician. On the 28th day of the fast he was given a cursory examination by a physician and was pronounced in good physical condition, despite rather extreme emaciation. Perhaps because he was accustomed to fasting, *C* found it unnecessary to take the twice-daily enema that the other *Ss* did as a matter of precaution. No biliousness nor other serious difficulty was encountered throughout his fast.

Nature of the tests. Seven tests in all were used during these experiments. They were selected with a view to making each task as difficult as possible. Where the task was not a difficult one the time-limit of performance was increased. None of the tests was of a purely muscular nature. Several were decidedly fatiguing. The reason will be evident in the following discussion.

Robinson and Herrman[7] have shown that 3 *Ss* were able to perform in tests about as well after 60–65 hrs. of insomnia as they did normally. These experimenters tested for an interval from 11–26 days preceding the sleepless period. They followed the period of sleeplessness with 4 or 5 days of testing. Relatively short tests were used; the total time for five tests took from forty-five minutes to an hour. Inasmuch as the *Ss* doubtless put forth more effort during the period of insomnia, it was thought that we might partially offset this possibility by using tests that were rather fatiguing.

In another experiment on sleep Robinson and Robinson[8] secured from their *Ss* ratings of the amount of effort that they felt they put forth in taking the tests. There were two groups, an insomnia and a control group. The experiment covered a period of three days. On the first day at the end

[7] E. S. Robinson and S. O. Herrman, Effects of loss of sleep, *J. Exper. Psychol.*, 5, 1922, 19-32.

[8] E. S. Robinson and F. R. Robinson, *ibid.*, 5, 1922, 93-100.

of the tests, 41% of the insomnia group felt that they had put forth their best effort. The night following was a sleepless one for the insomnia group. At the end of the tests, on the day after the sleepless night, 75% of them, as against 38% of the control group, felt that they had put forth their best effort. The insomnia group during the next 24 hours made up much of their lost sleep. On the third day only 49% of them, compared with 17% of the control group, felt they put forth their best effort during the tests.

These experimenters say: "The ratings of effort, considered in conjunction with those of tiredness, suggest that loss of sleep causes a general lowering of feeling tone accompanied by an increase of effort put forth upon a task. This increase of effort may so compensate for any ultimate loss in capacity that that loss may be relatively unobservable."

Further conviction along this line follows from the experiments of Kleitman[9] on the effects of prolonged sleeplessness. His *S*s went from 40 to 115 hours without sleep. He found no appreciable loss in the speed in naming 100 colors on a card. But when one *S* named 1200 colors (12 cards), Kleitman did obtain definite effects of insomnia.

After extensive investigation of various tests the following were selected.

(1) *Arithmetic.* Four hundred multiplication problems of two by two-place numbers were arranged from chance combinations of the digits 2–9 inclusive. Each two problems contained all of the digits. No digit occurred twice in the same problem. It was hoped that this would make the daily task fairly homogeneous. The problems containing the combinations 25 were eliminated because they suggested short-cuts. In a day's task no identical problems occurred. *A* solved 20 problems daily, and *B*, 40.

The multiplicand was first called out by *E*. It was repeated and written down by the *S*. In the same manner the multiplier was given, whereupon *E* immediately gave the signal, "Go." *S* gave the answer as quickly as possible without further writing. The correct answer was required. Time and errors were observed and recorded.

C thought he would be "forever" working off the effects of practice with so difficult a task. Consequently, and at his request, we constructed on the same principle 200 addition problems of 4x4-place dimensions. He performed 40 of these each day. The procedure was the same as for *A* and *B*, except that *C* wrote down the answers as he obtained them.

In this, as in the three tests that follow, *S* sat opposite *E* at a wide table. Between them was a screen, about 2 x 3 ft., supported on legs approximately 4 in. above the table top. It permitted *E* to observe what *S* wrote, or his general method of attacking a test. What *E* recorded was never observed by the *S*. Emotional distractions, due to the knowledge that one is being closely observed, are much more apt to be prevented by employing this simple device.

(2) *Writing.* The letters "ab" were written on uniform ruled paper for 20 min. as rapidly as possible. The *S* made a stroke on the line at the sound of a tap on the table every 30 sec. From the data obtained in this manner it was possible to construct fatigue curves for each of the periods. The errors were few enough to be negligible after several days of practice.

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[9] N. Kleitman, The effects of prolonged sleeplessness in man, *Amer. J. Physiol.*, 66, 1923, 67-92; 67, 1923, 141-151.

The results consisted of the number of letters written during the 20 min. of the test. This type of activity had previously been used in connection with another investigation in the Chicago Laboratory.[10]

(3) *Color-naming.* Twelve cards containing 100 colors each were prepared. The colors were one cm. square and placed one cm. apart on the cards. Green, red, yellow, black, and blue were the colors used, 20 of each to a card and in random order. The cards were presented face downward to *S* who turned them over and named the colors as rapidly as possible. The cards were not of equal difficulty; some of the combinations proved easier than others. For this reason and due to the fact that we presented the cards in regular order every day, we could not construct representative fatigue curves for this test. We recorded the time that it took to name the 100 colors on each card, however. Errors consisted in skipping colors or misnaming them. (Although we recorded errors, we shall not present them with our results; they seem not to have been influenced by the fast.) In case *S* corrected himself *E* made note of the fact by a symbol. This test was adapted from Kleitman's experiment on sleep.

(4) *Backward reading.* This test was suggested by the Robinson and Herrman experiment on insomnia, to which reference has already been made. Patrick and Gilbert were apparently its originators. Three magazine articles each for *A* and *B* were taken from a current periodical and mounted on cards. Each article was of 12-point type, 1000 letters in length. Each was read every third day.[11] *S* began at the bottom of the article and named from right to left the letters as they appeared in the words. He advanced to the top of the article by taking each successive line upwards. Results consisted of the time to read the article. Errors of both the corrected and uncorrected type were also recorded, but they seem to have been unimportant. Because several of the tests were trying on his eyes, *C* asked to be excused from taking this test.

(5) *Steadiness.* The device described in Whipple's *Manual*[12] was used to measure the changes in steadiness that took place. *S* was required to hold the pointer for 30 sec. each in the 3rd, 5th, 7th, and 9th holes of the metal plate. These holes were 16, 11, 9, and 7 sixty-fourths of an inch in diameter. The needle pointer was of standard design. The interval of rest between the pointings was 30 sec. Both hands were tested. The results consisted of the errors made in touching the walls of the metal plate with the pointer. These were recorded automatically by electrical connection with a clock-counter. A strong current was used, sufficient to 'carry through' with the least contact, but of insufficient intensity to injure the mechanism of the clock. To decrease noise the clock was embedded in a box of sawdust. Each click was reduced to a dull thud.

S was seated at a table in a comfortable manner, with body tilted slightly forward. He was not permitted to hold his elbow against his side. When he was in position a switch was thrown at the signal 'go,' completing the circuit. In the interval of rest *S* always held the pointer in the hand that was being tested. Otherwise he disposed of his time as he desired.

(6) *Visual acuity.* In lieu of some standard test on visual acuity that fulfilled the requirements of this experiment, the following was constructed. Five rows each of 12-, 10-, 8-, 6-, and 5-point type, 25 letters to a row, were printed on a card. Letters that looked somewhat alike appeared next to each other in a mixed fashion. For instance, the letters P B R and O D G C appeared in different patterns, unspaced and in capitals, along with

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[10] E. S. Robinson and A. G. Bills, Two factors in the work decrement, *J. Exper. Psychol.*, 9, 1926, 415-443.

[11] I cannot commend this procedure for eliminating the effects of practice. Only one card should have been used.

[12] G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 155-160.

most of the other letters of the alphabet. The purpose was to have a test of some length where fine visual discrimination could be measured.

This card was placed under a light in a dark-room. A reflector shielded the light, permitting no glare to the *S*s vision. A rheostat was inserted to cut down the current. A translucent white light ('milk glass') of 50-candle-power, using 16/50 amperes, was employed. *S* sat with his head against a head-rest; his eyes were 42 in. from the card. This distance and the amount of current used were determined by the available space in the dark-room. *E*, with the aid of a flashlight behind the curtain, checked *S*'s performance on a duplicate of the test.

S was asked to read the letters as steadily as possible, taking only the second, third, and fourth lines of each size of type.[13] The test began with the largest type and proceeded downward, including the smallest type; *S* read from left to right. He indicated by a short gutteral "umph" a letter that he could not identify, and proceeded to the next. *E*, with the aid of a long handle, opened and closed a small door covering the card, when necessary, during the test. A few seconds were taken between readings on each size of type to record time and omissions. *E* was never visible to *S* during the test.

This test required some time to prepare, and as a result did not get the benefits of a long practice period prior to the fasts. It was completed and used for the first time about 10 days before the first fast. Without anticipating our findings, we may say that the results secured were decisive, in view of the post-fast performances of all *S*s in this test.

(7) *Equilibrium.* This test is another measure of steadiness, in this instance of the ability to stand at 'attention.' A device known as the ataxiagraph, mentioned and illustrated by Doll,[14] was constructed. It consisted of a glass cylinder fastened upright to a head-band. A delicate spring was enclosed within the cylinder. From the end of the spring a stylus projected through a small hole in the stoppered cylinder.

This 'crown' was buckled on the head, the stylus pointing upward from the forehead. A fixed support held a metal plate upon which sagged rather loosely a smoked paper. When *S* was placed in position this plate was lowered on a standard until the stylus touched the smoked paper. It thus recorded the movements of the head or body by a tiny mark upon the smoked surface.

The position of the *S* was approximately at 'attention.' The feet formed an angle of 45–50°, the heels were together, and the body comfortably erect. The arms extended downward by the sides, palms inward. *S* stood in this position for 2 min. blindfolded and 2 min. with vision. Between the conditions there was a rest interval of 2 min. during which the *S* was seated. The order of the conditions was alternated from day to day.

The extreme distances from left to right and back and forth over which the stylus moved constituted the measurements for this test. These measurements were taken in mm. by the use of a needle-point divider, the spread being read from a steel scale.

General procedure. Preceding the experiment every *S* agreed that he would do his best very day throughout the entire experiment and endeavor to keep an open mind as to the outcome. No *S* was permitted to know his own performances from day to day on any test.

A and *B* performed the first four tests, namely, arithmetic, writing, color naming, and backward reading, in their home. They acted as *S* and *E* in turn for each other in all tests except that of visual acuity. In order to make this possible duplicate forms of some of the tests were constructed.

[13] Thus a line was always immediately above and below the one being read, a fact which would not have been true if the first and last lines had been read.

[14] E. A. Doll, *Clinical Studies in Feeblemindedness*, 1917, facing p. 66.

Different materials were used for backward reading. While *S* read the material backward, E followed, using a copy on which the material was read forward. *E*'s copy was in large type and the words were not spaced, so that it looked to the *E* like a pied paragraph.

These 2 *S*s used the same arithmetic problems, but rarely solved the same set from the 400 on any given day. Whenever this did happen, as it was bound to when *A* solved 20 and *B* solved 40 problems each day, it was rarely, if ever, noticed by the person acting as *S* after having acted first as *E*. These *S*s never memorized the answers to any problems.

For the color-naming test the names of the colors were typed in large print for the benefit of *E*. This made it easy to follow *S*'s reactions.

It can be seen that these three tests are probably the only ones wherein *S* could have any advantage by having acted as *E*. We believe that the precautions explained above reduced to a minimum, if indeed they did not entirely eliminate, any advantage that might accrue to one acting the rôles of both *S* and *E*.

The writer always acted as *E* for *C* except in the test of visual acuity. A graduate student in psychology, Miss Marian Geohegan, to whom we are greatly indebted, acted the part of *E* in this test. All tests except the four given at home, were given in the Psychological Laboratory.

The results secured during the practice period were graphed by the *E*. The four major tests, arithmetic, writing, color-naming and backward reading were the ones especially observed. The fast was not begun in any case until the *S* had apparently reached a level of performance and remained there for several days. Dr. E. S. Robinson advised *E* as to the time when the fast should begin. The experiments were thus subjected to better control by having an experienced referee who was not a subject.

As a matter of convenience the results from testing *C* are graphed as if the tests were given every other day. As we have explained previously, only 48 out of the 110 days of the experiment were testing days, so this is not quite true.

Results

(1) *General tendency to lose.* In the four major tests there was a general tendency for the *S*s to 'perform' at a lower level of efficiency during the period of fasting. This tendency was more conspicuous in the case of the longest fast, but was generally characteristic of the shorter ones as well.

Figs. 1-5 show the performances of the 3 *S*s throughout the experiment. In estimating the effects of the fast the early portions of the graphs can be ignored; these sections of the curves illustrate mainly the marked influence of practice upon the activities in question. When, hereafter, we make reference to the pre-fast performance, we are considering the relatively flat sections of the curves just preceding the fast. The cross-bar designates the point at which we assume practice to have become a minor factor. The broken portion of each curve represents performance during the fast.

Fig. 1 shows that *C* lost considerably in the time required to perform 40 addition problems when enduring the fast in comparison with the pre-fast and post-fast performances. For about 15 days he performed as well during the pre-fast period,

but during the next 10 days his rate of performance was definitely decreased. During the last week of the fast he returned about to the level of the pre-fast performance.

The curves for *A* and *B* show little more than daily fluctuations in performance during the fast. Especially is this true in comparison with the pre-fast section of the curves. The stimulating effects of food after a fast, indicated by most of the tests, will be discussed in a later section.

Fig. 2 shows that both *A* and *C* suffered a distinct loss during the fast in ability to write, as measured by the number of letters

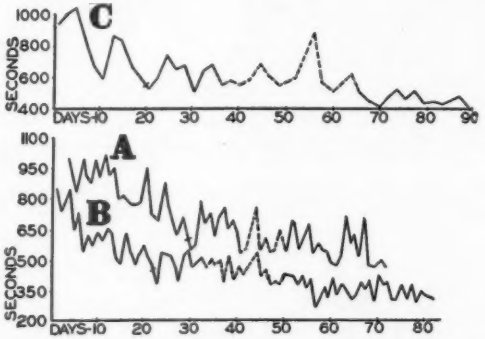

FIG. 1. ARITHMETIC. Performances of *A*, *B*, and *C*, before, during and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins with black bar; fast is in dotted line

written. After the first 2 days of fasting the output of these two *S*s gradually decreased. On the last 2 days of the fast *A* showed a tendency to recover some of the losses sustained. Toward the close of the fast *B* suffered a most marked loss of output. His strong physique doubtless was responsible for his ability to perform at a high level for almost 2 weeks during the fast.

Fig. 3 shows very clearly that *C* took a much longer time to name 1200 colors during his fast than he did previous to it or afterward. The differences between the pre-fast performances and those during the fast are much more marked in his case than they are for *A* and *B*. The after-fast acceleration again was great with this *S*. The only conspicuous result shown by the curve for *B* is the after-fast acceleration. The performances of *A* show little variation from normal.

No graphs are presented for backward reading. Of the two *S*s taking this test, *A* reacted more slowly during the fast than

before or afterward, while *B* was apparently unaffected by the fast, though he did show a post-fast improvement. These facts are clearly enough shown in Tables I and II.

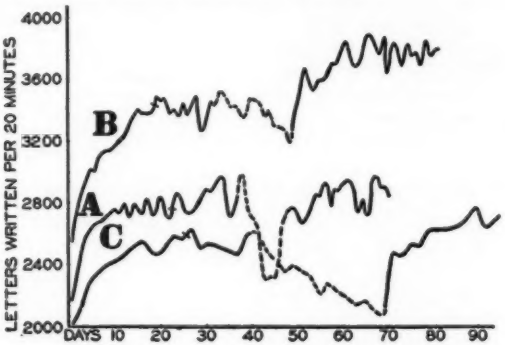

FIG. 2. WRITING. *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

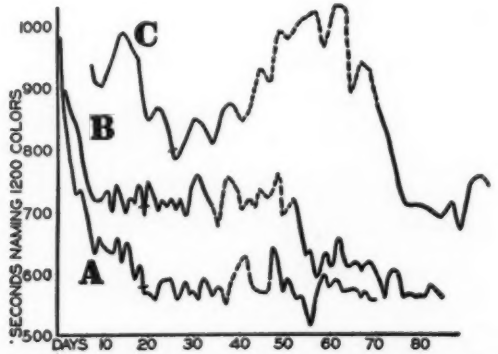

FIG. 3. COLOR NAMING. Performances of *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

Fig. 4 shows the relative time consumed in naming the letters in the visual acuity test. Every *S* proceeded at his own rate during this test. The graphs show very clearly that all *S*s took more time to perform during the fast than either before or afterward, in spite of the fact that the task performed after

the fast was somewhat greater than that preceding or during the fast, *i.e.* the total number of letters read per test was greater after the fast.

Tables I, II, and III show the average performances of *A*, *B*, and *C* in all of the tests. The performances are grouped into the three main periods of experimentation—the pre-fast, the fast, and the post-fast. The pre-fast is that period before the fast but following the pronounced practice effects of the very early performances (see cross-bars on graphs). The averages show a general tendency of the *S*s to lose in efficiency during the fasts. The marked exceptions to this tendency will be given separate consideration.

TABLE I

PERFORMANCES OF *A* IN TESTS PRECEDING, DURING AND FOLLOWING A 10-DAY FAST.

Abbreviations: ″ = seconds; c = errors (contacts); mm = millimeters

Tests		Pre-fast	Med. Dev.	Fast	Med Dev.	Post-fast	Med. Dev.
1. Multiplication		675.0″	44.5	609.3	46.5	574.5	70.2
2. Writing (letters)		2813.4	68.6	2576.71	69.5	2834.3	84.7
3. Color-naming		575.3″	14.3	584.2	16.2	578.1	18.9
4. Backward reading		333.2″	6.2	364.7	15.5	309.3	18.3
5. Steadiness (hands)	Right	14.45c	6.4	13.8	5.38	11.52	6.5
	Left	15.55c	4.4	18.5	5.00	13.56	3.5
	Total	30.00c	6.0	32.3	12.88	25.08	10.1
6. Visual acuity	Letters	356.0	3.0	355.0	2.0	358.9	2.1
	Time	338.0″	8.0	394.0	34.5	316.4	32.6
7. Equilibrium Blindfold	L-R	37.7mm	2.3	34.4	9.0	38.9	8.9
	B-F	44.8mm	5.2	46.1	6.4	58.0	8.0
Seeing	L-R	30.5mm	9.5	29.8	3.8	30.9	6.9
	B-F	36.1mm	13.1	36.8	5.3	45.5	10.5
Totals	Blindfold	82.5mm	9.5	80.5	3.5	96.9	16.9
	Seeing	66.7mm	12.7	66.5	9.0	76.4	13.6

(2) *Increase in steadiness.* Fig. 5 shows graphically the results of the Whipple test of steadiness. The curves have different ordinates to prevent superposition but have the same abscissae. The results for both hands are combined in the interest of simplicity. Separate graphs of the results from testing the two hands showed no consistent differences between the performances of the right and of the left hands. The averages show only slight differences between the two hands, as can be seen from Tables I–III. The curves for *B* and *C* will be discussed first.

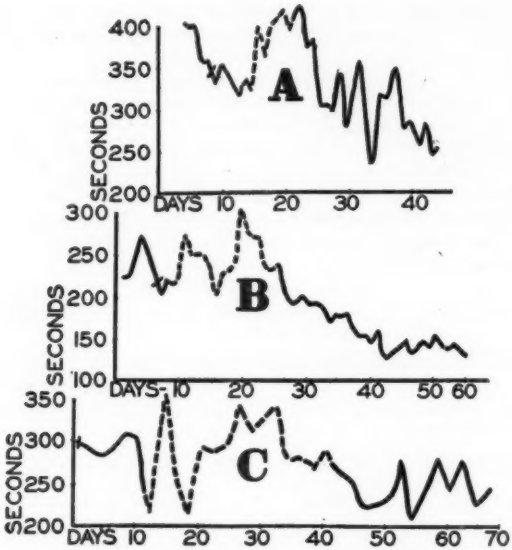

FIG. 4. VISUAL ACUITY. Performances of *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

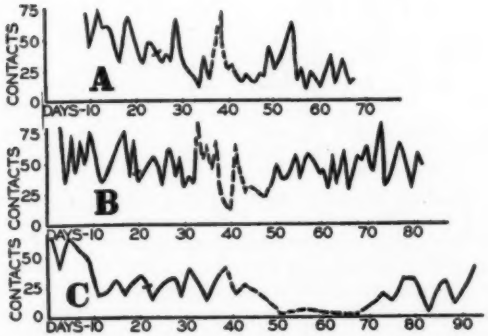

FIG. 5. STEADINESS. Performances of *A*, *B*, and *C*, before, during, and after a 10-, 17-, and 33-day fast, respectively. Pre-fast begins at black bar; fast is broken line

B and *C* show a gain in steadiness when the results during the fasts are compared with the other conditions. Due to the variations in the curve for *B* the gain is not so clearly shown as it is for *C*. The averages in Table II show that *B* did make a small gain during the fast in this test, though his gain is not statistically certain.

The gain of *C* is very conspicuously shown by the graph, Fig. 5. After the first few days (tests were given about every other day) he showed a steady increase approaching almost zero errors as the fast continued. He remained at this low level

TABLE II

PERFORMANCES OF *B* IN TESTS PRECEDING, DURING AND FOLLOWING A 17-DAY FAST.

Abbreviations: ″ = seconds; c = errors (contacts); mm = millimeters

Tests			Pre-fast	Med. Dev.	Fast	Med. Dev.	Post-fast.	Med. Dev.
1. Multiplication			499.9″	26.9	459.9	44.1	369.9	38.9
2. Writing (letters)			3413.7	51.5	3380.9	82.9	3728.2	87.8
3. Color-naming			722.9″	15.9	724.7	17.3	610.6	14.4
4. Backward reading			264.5″	6.5	262.4	7.6	222.2	3.8
5. Steadiness (hands)		Right	25.2c	3.8	19.0	11.0	22.9	4.9
		Left	19.2c	5.8	23.1	8.1	24.5	5.5
		Total	44.4c	11.4	42.1	17.1	47.4	9.6
6. Visual acuity		Letters	374.4	0.4	373.4	1.6	374.9	0.1
		Time	213.7″	9.3	249.2	21.2	159.6	21.0
7. Equilibrium	Blindfold	L-R	33.9mm	12.0	30.1	5.1	35.1	7.5
		B-F	34.1mm	5.1	40.2	8.8	44.2	8.0
	Seeing	L-R	29.1mm	5.9	28.5	3.5	30.2	8.7
		B-F	32.7mm	3.7	38.2	10.2	39.3	7.3
	Totals	Blindfold	68.0mm	13.5	71.0	11.0	79.2	11.5
		Seeing	61.9mm	8.8	66.7	14.3	69.6	14.6

until the completion of the fast when, as can be seen by the curve, he became less steady. It was impossible to keep this *S* from knowing that he did better during the fast. Although *S* mentioned the fact several times during the fast, *E* never discussed the matter with him.

Table III shows that *C* made on the average about 14 less errors during the fast than before it. When compared with the post-fast the errors are about 10 less. The corresponding gain for *B* (Table II) is not so marked.

Table I shows that *A*, who underwent the shortest fast, suffered a loss in steadiness during the fast. Taking the average

of the first 7 days of fasting, every *S* showed a distinct loss over the two other conditions. Had we stopped the tests at the end of one week of fasting, and secured the same data for the post-fast performances of all 3 *Ss*, we could have said that the fast caused a marked falling off in steadiness for every one. The curves show that all *Ss* increased in steadiness after the first week of fasting, except for one bad day for *B*.

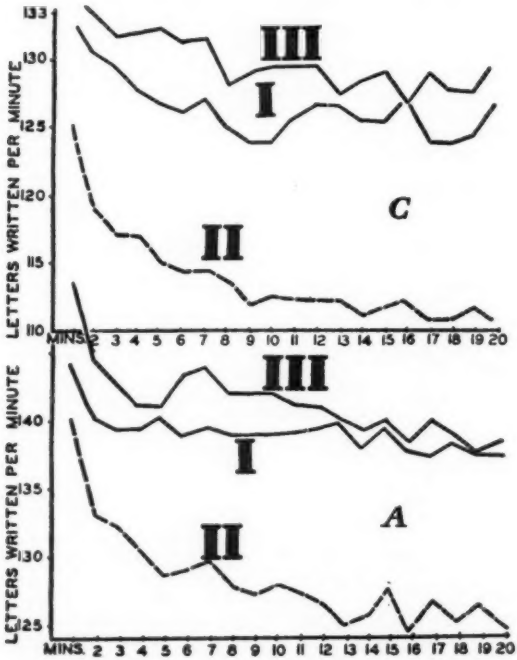

FIG. 6. WORK-CURVES. Curves of *A* and *C* for writing in the pre-fast (I), the fast (II), and the post-fast (III) periods

The other form of steadiness, *i.e.* equilibrium, was little affected by the fast. See Tables I-II.

(3) *Fatiguability.* Fig. 6 shows the work-curves of *A* for the writing tests. One curve is shown for every one of the three phases of the experiment. These curves show the general ten-

dency of *S* to become more fatigued as the day's task progressed. The pre-fast curve shows that he performed at a level considerably higher than that of the fast, but the important fact illustrated is the greater fatiguability during the fast. *A* began writing about 140 letters per min., on an average, and finished the day's task at the rate of 125 letters per min. During the pre-fast and post-fast the corresponding losses were a little more than half this amount.

The curves for *C*, Fig. 6, represent almost exactly the same thing, except for his tendency to perform at a little higher level during the last minute.

TABLE III

PERFORMANCES OF *C* IN TESTS PRECEDING, DURING AND FOLLOWING A 33-DAY FAST.

Abbreviations: ″ = seconds; c = errors (contacts); mm = millimeters

Tests			Pre-fast	Med. Dev.	Fast	Med. Dev.	Post-fast.	Med. Dev.
1. Addition			568.3″	37.2	559.5	40.5	451.3	19.3
2. Writing (letters)			2533.0	53.0	2306.9	127.1	2619.6	74.4
3. Color-naming			841.8″	28.3	975.3	66.3	742.5	30.5
4. Steadiness (hands)		Right	12.9c	7.0	5.0	4.0	9.2	3.8
		Left	11.8c	4.0	5.8	4.8	11.2	4.2
		Total	24.7c	8.0	10.8	8.8	20.4	7.4
5. Visual acuity		Letters	357.3	4.0	361.0	3.0	365.5	5.0
		Time	299.5″	9.5	290.0	25.0	247.1	19.1
6. Equilibrium	Blindfold	L-R	25.7mm	2.7	25.6	6.6	24.4	4.4
		B-F	41.1mm	1.9	32.9	6.1	32.5	4.5
	Seeing	L-R	22.4mm	5.4	22.5	5.5	20.2	4.2
		B-F	32.2mm	9.2	32.5	7.5	37.5	4.5
	Totals	Blindfold	66.8mm	4.2	58.5	9.5	56.9	4.9
		Seeing	54.6mm	10.6	55.0	11.0	57.7	3.7

The curves for *B*, which are not shown, indicate that his performances during the pre-fast and the fast went on at about the same level in this test. He showed less susceptibility to fatigue than the others.

(4) *After-fast acceleration.* With few exceptions, the after-fast performances were on a higher level of efficiency than either of the other conditions. This relationship is true of all tests except those in which a gain was made by the *S*s during the fasts. It also applies more to *B* and *C*, who endured the long fasts, than to *A*, who fasted only 10 days. Although *A* showed a gain in the post-fast over the two other conditions in all but the

color-naming and equilibrium tests, her gains are not so outstanding as those of the two other *S*s. The curves that most conspicuously show the acceleration of after-fast performances are Figs. 2 to 4.

It may be possible that the effects of practice are still operating to some extent in some of the tests. But Figs. 2, 3, and 4 seem to show an additional effect, especially for *B* and *C*. Fig. 2 shows that these *S*s performed in writing almost immediately after the fasts at a higher level than ever before. Fig. 3 shows that these *S*s began naming colors a few days after the resumption of food at a rate never before attained. *A*, and especially *B*, spent much less time in reading more letters, as shown by Fig. 4, immediately after the fast was concluded. These facts lead us to believe that there is a marked after-fast acceleration due to the stimulating effects of food.

Carlson[15] relates some of the after-effects of fasting for a few days at a time. Speaking of his physical and mental condition after a 5-day fast he says, "the writer felt as if he had had a month's vacation in the mountains. The mind was unusually clear and a larger amount of physical and mental work was accomplished without fatigue." All of our *S*s spoke of the unusually heightened feelings they experienced during the after-fast period.

(5) *Decrease in weight.* Table IV shows the weights of the *S*s at the beginning and at the close of the fasts.

TABLE IV

WEIGHT LOSSES DURING FAST

S	Weight		Loss		
	at beginning	at close	pounds	%	Pounds per day
A	121.3	109.1	12.2	10.1	1.22
B	166.3	144.1	22.2	13.4	1.31
C	148.7	118.6	30.1	20.3	0.91

Twenty-three days after the fast *A* had not regained her full weight. Since this was June 19, and she was only 4.7 lbs. underweight, it may be said that she was not far from normal. On the 19th day after concluding his fast *B* was 0.5 lbs. over-weight. *C* was 0.2 lbs. under-weight 29 days after concluding his fast, but since this was on July 29, almost mid-summer, it may be said that he was slightly over-weight.

CASUAL OBSERVATIONS

Increase in smell sensitivity. The most outstanding observation made by the *S*s enduring these longer fasts, was the marked increase in the ability to detect odors during the fasts. Food, we believe, could be detected at a greater distance. *C*

[15] A. J. Carlson, *Control of Hunger in Health and Disease*, 1916, 137.

reported that perfumes and even body odors were keenly noticed by him when he met or passed people on the streets during his many walking trips while fasting. *B* refused to drink the city water after a few days of fasting because of the offensive odor of the chlorine in it, a fact rarely noticed at other times. (The office of the City Engineer informs me that a chlorine gas is used in the city water.) *B* purchased a supply of spring water to use for drinking purposes during the last 10 days of the fast.

Marsh hints at results of this nature when he says that taste and smell "were tested for two weeks and dropped, due to the delay entailed; but the results would have proven as interesting as any, had this section of our plan been completed."[16] Langfeld says that Levanzin called his attention to the fact that "heightened sensitivity for odors during his first fast made walking on the streets positively unpleasant."[17]

It is a well-known fact that hunters deny food to their dogs for several hours preceding a hunt. This is a common practice with fox hunters in a certain section of the South. The popular opinion is that the animals do better when the stomach is empty. Without denying the value of hunger as a stimulus to action, it seems probable that the animal's olfactory sense is much keener too. Thus the opinion that a hungry animal can follow a 'cold' trail better than when he is well-fed may, after all, have some basis in fact.

It was originally intended to secure some measure of smell sensitivity during the experiment, but it was difficult to get a test that met all of the requirements of the problem without over-loading the program. It is hoped that objective results will be obtained by some experimenter in the future on this apparent increase in sensitivity to odors.

Dreams. Dreams were prominent in only the longest fast. Only a few were reported by *A* and *B*. These were either very hazy and disconnected or were remotely related to food. *C* reported several dreams about food after three weeks of fasting, but none was very striking.

Sexual feelings. About a week after *C* had concluded his fast, the writer asked him about the effects of fasting on the sexual impulses. He substantiated what the writer had already observed, that food after a fast of some length tended to accelerate the activities of the sexual organs. *C* had noticed this, also, at the conclusion of previous fasts.

Someone has suggested that this condition is due to eating considerably more meat than ordinarily. It is quite possible that an increased meat diet will serve thus as a stimulant after a fast. It is more reasonable to assume that an increased sexual tone would naturally accompany the greatly increased body-tone that normally follows a fast. That the entire body-tone was heightened after the fast was reported by all 3 *Ss*. This increase in sexual tone should also be thought of in relationship to the high mental efficiency shown by certain of the tests.

[16] *Op. cit.*, 438. [17] *Op. cit.*, 49.

Miscellaneous. The bodily temperature of *A* and *B* remained almost unchanged throughout the fasts. Respiration increased slightly. Both measurements were taken after 30 min. rest in a prone position. As *C* had endured several fasts it was thought unnecessary to observe him so carefully in these respects.

Every visit to the laboratory and the return home required the *S*s to climb several flights of stairs. After about 10 days of fasting the climbing did not cause the limbs to ache as much as it did during the earlier part of the fasts.

Only one *S* was an occasional smoker. He smoked freely during the first two days of the fast, began to lose the taste for tobacco on the third, and gave it up completely on the fourth day.

Conclusions

The conclusions reached as a result of this experiment are as follows.

(1) The so-called 'mental' out-put is generally decreased during a long fast.

(2) A fast of more than a week's duration definitely increases steadiness of hand.

(3) A subject fatigues more rapidly during a fast than normally, when the task is of considerable length and homogeneity.

(4) Performance at some tasks after a long fast is much more efficient than normally, an effect which can hardly be attributed to practice alone.

(5) In casual observations it was noticed (a) that the smell sensitivity is greatly increased during a long fast; and (b) that sex feelings in males are considerably accentuated when eating is resumed after a long fast.

Effects of Short Fasts

In order to test the effects of short fasts *A* and *B* of the preceding experiment, and a new subject, *D*, a Chinese male graduate student, fasted one day out of every six. *A* and *B* fasted 7 times and *D* fasted 5. A period of about three weeks practice preceded the fasts. Arithmetic, writing, and the two tests of steadiness were used. These are identical with those used in the preceding experiment, and were given in the same way.

The results on the days of fasting were compared with those secured on the 3 days previous to and the 3 days following the days of fasting. Each fast was endured on a different day of the week.

No definite results were secured. On some fast days the *S*s did better than on the pre-fast or post-fast days, and on others decidedly poorer. It was almost impossible for the *S*s to main-

tain the same attitude toward the experiment on the day of the fast as on other days. This conscious factor undoubtedly did much toward obscuring the real results.

Wada[18] has shown a close relation between hunger contractions of the stomach and best performances in mental tests. She tested almost continuously for 10 to 12 hrs., after the *Ss* had gone without food for the greater part of a day. Almost without exception, the best results accompanied hunger contractions of the stomach. Since our tests were all performed in about 1½ hrs. work a day it is readily seen where our difficulty lies. Although our *Ss* were, in a general sense, hungry on the foodless days, we can have no assurance that there were hunger contractions during the actual test period.

We see no way of reconciling our negative results with the positive results obtained from short fasts by Weygandt and by Aschaffenberg.[19] Both of these students found short fasts to have markedly deleterious effects.

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[18]Tomi Wada, An experimental study of hunger in its relation to activity, *Arch. Psychol.* 8, 1922, No. 57.

[19]Cited by Langfeld, *op. cit.*, 47.

THE EFFECT OF BRIGHTNESS IN THE RANGE OF ATTENTION EXPERIMENT

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INTRODUCTION

From the point of view of structural psychology, sensation is to be defined in terms of its attributes. The attributes common to all sensations are quality, intensity, duration and clearness,[1] that these have independent status, is established by both logical and introspective analysis. The only possible confusion—that between intensity and clearness—is conclusively resolved by appeal to the familiar fact that attention to an intensively weak component of a sensation-complex brings it out clear and vivid in consciousness, without appreciably altering its relative intensity. So much is clear.

Whether independent status implies independent variability is a controversial question.[2] The ultimate answer to this problem can be arrived at only by appeal to actual experiment, supplemented by introspective report.

The object of this experiment is to arrive at a preliminary determination of the influence upon clearness produced by variation of stimulus-intensity. It were indeed a fond hope to expect anything more than a tentative and provisional answer to our question, based, as the results necessarily are, upon the highly specialized conditions of our experimental setting. Nevertheless, it is the writer's belief that conclusions, validly drawn from careful and accurate experimental results, cannot but throw some light upon this moot question.

Many ambiguous results have been obtained from experiments to determine whether or not intensity is increased by attention. With this phase we are not directly concerned in this study. Our principal concern is with a comparison of the clearness of perception resulting from objectively varied intensities of stimulus, and we have adopted, as our means of approach, the familiar problem of statistically calculated limens for visual apprehension of dots—the old so-called range of attention.

The writer is well aware of the possibility that the question may be raised as to the validity of his basic assumption, viz., that the limen values for apprehension of different stimulus-intensities afford a true measure-

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[1] Külpe, Dunlap, Warren, and others omit 'clearness' from the list of attributes of sensation and include 'extensity.' Külpe does not distinguish clearness from intensity; he says, "intensity itself is the property of sensation which enables us to compare it with others in respect of vividness." (*Outlines of Psychology*, 1895, 30.)

[2] Cf. E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 9 f.

ment of clearness of apprehension. The distinction and independent variability between attributive clearness and cognitive clearness, pointed out by Dallenbach,[3] are confirmed in this study. Despite this complication, however, we are convinced that our assumption is justified not alone by the insurmountable practical difficulty of maintaining constancy of attitude,[4] but also by the results themselves.

In regard to the problem of the relationship between intensity and clearness, Pillsbury states: "Whether the change in clearness is identical with, or dependent upon, change in intensity is as yet open to controversy."[5]

Titchener asks, "Is clearness ever independent of intensity?" and replies, "On the whole . . . the trend of psychological belief just now seems to favor an interdependence of the two attributes."[6]

The phenomenon of consciousness, in the large, has one fundamental characteristic: consciousness of something, whether it be sensation, image, feeling, idea, or perception. That is, there are always two levels of clearness to every conscious experience—a something which stands out and a background from which it stands out. This is the basis of the two-level concept of consciousness. The processes which stand out as relatively more vivid and distinct are called the focus, while the surrounding mass of unclear or obscure processes is termed the margin, fringe or background of consciousness. Whether more than one level of clearness exists in the background does not interest us here; but the possible existence of more than one clearness-level within the focal area is inextricably bound up with studies of the range of apprehension.

In the more recent tachistoscopic investigations of the so-called range of attention, just such distinction has been made between relative levels of clearness within the 'upper-level' or focal area of consciousness.

Fernberger[7] came to the same conclusion that had been reached earlier by both Whipple[8] and Dallenbach,[9] viz. that 'range of attention,' as applied in the older writings, was a misnomer and rightly should be called 'range of apprehension.' This conclusion was based on the observed fact that more than one degree of clearness was discriminated by *Os* within the focal consciousness.

From the structuralist point of view, there is general agreement that the term 'attention' is practically synonymous with maximal clearness,[10] the term 'apprehension'[11] is applied to lesser degrees of clearness found within the focal area, distinct from the definitely and sharply demarcated obscurity of the margin or background of consciousness.

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[3]K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 229. N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 248.

[4]E. S. Rudisill, Constancy of attitude in weight perception, this JOURNAL, 36, 1925, 562-587.

[5]W. B. Pillsbury, *Attention*, 1908.

[6]E. B. Titchener, *A Text-Book of Psychology*, 1909, 212.

[7]S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 121-133.

[8]G. M. Whipple, *A Manual of Physical and Mental Tests*, I, 1924, 278.

[9]K. M. Dallenbach, The effect of practice upon visual apprehension in school children, *J. Educ. Psychol.*, 5, 1914, 321-334, 387-404; The effect of practice upon visual apprehension in the feebleminded, *ibid.*, 10, 1919, 61-82; especially, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 228 f.

[10]Titchener (*Text-Book of Psychology*, 267) states, "in the last resort, and in its simplest terms, attention is identical with sensory clearness." Breese (*Psychology*, 1917, 59) states, "Attention is clear and central consciousness." Pillsbury (*Attention*, 1908, 11) states, "Attention is fundamentally a change in clearness of some one phase or aspect of a mental process." Hunter (*General Psychology*, 1919, 114) says, "Attention is the clearness into and out of which objects move."

[11]Dallenbach, *opp. cit.*; Fernberger, *op. cit.*; Whipple, *op. cit.*; but see Dallenbach, Dr. Oberly on 'The range for visual attention, cognition and apprehension,' this JOURNAL, 36, 1925, 154-156.

Oberly,[12] starting with this distinction, added a third conscious pattern, which received introspective justification. The first process, which he designated as *Immediate*, was introspectively characterized by immediacy of apprehension, coupled with equal and maximal clearness of all elements; the second process, which he called *Grouped*, was similarly characterized by immediacy, but coupled with more than one level of clearness; the third process, which he called *Counted*, was marked by a temporal succession, due to a mediating process of re-imaging, coupled with more than one level of clearness. To these three conscious processes or patterns, Oberly applied the systematic categories: Attention, Cognition, and Apprehension, respectively.

In the present problem, the writer has adopted Oberly's method of procedure with but slight variation, believing it to be well adapted for application to the intensity-clearness problem. The necessary variation of stimulus-intensity was introduced by using three different brightnesses of dots (black, dark gray and light gray) on a white background; Oberly used only black on white. In an earlier experiment, Fernberger[13] had obtained intensity-differences by using black dots on a white ground, and black dots on a medium-gray ground.

As stated above, the purpose of this experiment was to determine whether or not any constant relation exists between stimulus-intensity and degree of perceptual clearness. We assumed that such relationship would appear in a comparison of the values of the limens and the shapes of the curves of the psychometric functions for the different stimulus-intensities and under the different patterns of conscious process. Such objective evidence was amplified by frequent introspections from the *Os*, in the hope that information might be obtained in addition to that afforded by the individual judgments of the stimuli.

Experiments

Apparatus. The apparatus used in this experiment for exposing the stimulus material was a Whipple disk tachistoscope.[14] The adjustment of weights was regulated to give an exposure-period of 75 sigma, calibrated by the tuning-fork method. The pre- and post-exposure field was of uniform black, except for a white fixation mark at the center. The only illumination was supplied by a 25-watt lamp.

The stimuli consisted of 5 sets of white cards, 88 mm. sq.[15] on which were pasted in haphazard order dots, 6.5 mm. in diam., and ranging from 2 to 12 in number. There were 33 cards in each set, 11 of which had *black* dots, 11 *dark gray* and 11 *light gray*, each brightness group ranging from 2

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[12]H. S. Oberly, The range for visual attention, cognition and apprehension, this Journal, 35, 1924, 332-352.

[13]Fernberger, *op. cit.*

[14]For a description of this apparatus see G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1924, 264.

[15]Hurlock's 8-ply Oil Crest illustrating board was used.

to 12 dots. Thus there were in each set, 3 cards of each denomination: *one black, one dark gray,* and *one light gray,* all arranged in different haphazard orders. The arrangements were different for all five sets.[16]

These stimuli will be referred to as Bk, D, and L, respectively. The contrast of these three brightnesses on the white background of the cards thus afforded *three intensities of stimulus.*

Method of procedure. The tachistoscope was set up on a low table and the *O* seated so that his eyes were 2 m. from the exposure field, which at this distance, lay entirely in foveal vision. Instructions, read to the *O* at the beginning of each observation period, were as follows:

"You will be shown successively, for a very short exposure, cards which contain a varying number of dots on a white background. Immediately after the exposure you will report verbally:

"(1) The number of dots which you have apprehended.

"(2) The degree of assurance or certainty of your judgment, using this scale for your report: 5—if it is a 100-to-1 bet, 4—if it is a 5-to-1 bet, 3—if it is a 1-to-1 bet, 2—if it is a 1-5 bet, 1—if it is a 'guess' or 'do not know.'

"(3) The method of determining your report, whether by *Immediate judgment, i.e.* ungrouped, by *Grouping* of dots, or by *Counting.*"

These instructions are the same as those used by Oberly,[17] except for the omission of the word 'black' before 'dots' in the first sentence.

Accepting Oberly's results and conclusions as our starting-point, we defined the methods of determining reports as follows: *Immediate*—where all the dots "were perceived in a single flash of consciousness, with an equal and high degree of clearness." *Grouped*—where immediate perception occurred, as above, "but owing to different degrees of clearness, there was an attentional grouping of the stimulus dots." (Concerning this characterization we shall have more to say later.) *Counted*—where "some subsequent re-imaging of the stimulus material" provided the basis of the report.

In the Results we have presented, under the category of Attention, limens and coefficients of precision calculated from the correct judgments, based on the Immediate method. Under Cognition, we have included measures based on the Immediate method plus the Grouped method, *i.e.* those showing more than one level of clearness. Under Apprehension, we include measures based on all three methods: *Immediate, Grouped,* and *Counted.*

As soon as the *O*'s gaze was directed toward the fixation-point of the pre-exposure field, the warning signal was given. This consisted of a 'Ready,' followed at an interval of approximately 1 sec. by a 'Now;' the catch was then released, which brought the exposure of the stimulus-card after the lapse of another second. Thus the actual stimulation followed the first signal at an interval of approximately 2 sec., allowing for maximal concentration of attention. The stimulus was exposed, as stated above, for a period of 75 sigma.

The *O*'s 3-fold judgment was recorded as soon as given; then the next card in order was exposed as before, preceded by the signal. In order to obviate, as far as possible, unconscious memorization of individual cards or their sequence, the 5 sets of cards were shown in order from I to V, the cards being placed in a given position, which was called the 'A' position. At the completion of the 5 series (165 cards) in this position, the cards were

[16]The papers from which these dots were cut were obtained from Milton Bradley Co., and are listed by them as Black, Neutral Gray No. 1 (Light), and Neutral Gray No. 2 (Dark). Matched on a color-mixer, they were found to contain the following: Neutral Gray No. 1—black 63%, white 35%, yellow 2% (this was our 'light'); Neutral Gray No. 2—black 73%, white 25%, yellow 2% (this was our 'dark'); Black—black 100%.

[17]Oberly, *op. cit.*

turned to the left through an angle of 90°, and all 5 series gone through in this 'B' position; then, similarly with the 'C' and 'D' positions. In this way, a given card was shown in a certain position only after the exposure of 659 other stimuli. Another variation was provided by starting at different points in the sequence of the individual set.

The experimental periods were so arranged that no *O* ever observed more than three days in one week, nor longer than one hour at a sitting. A rest period of 10 or 15 min. was always allowed after each 20 or 30 min. observation period.

Each of the *O*s was given a period of preliminary training to familiarize him thoroughly with the procedure, so that the complex judgment had become perfectly mechanized before recording was begun.

The first series of observations was begun in February, 1925, and completed in June, 1925. The second series began in October and ended in December of the same year.

Each series consisted of 10 exposures of each of the 5 sets (of 33 cards each). Thus every value of stimulus from 2 to 12 dots, for each of the 3 intensities (Black, Dark Gray, and Light Gray) was exposed 50 times in each series, making a total of 3300 observations for each *O*.

Observations were made, for the most part, in a photographic darkroom. Toward the latter part of the second series we were compelled to transfer our apparatus to a large store-room of the department. Although a small amount of light entered from a distant window, careful scrutiny of the results gave no evidence of any change other than the normal improvement incident to practice, which had been apparent from the beginning.

Observers. The four *O*s, all members of the staff, were: Samuel W. Fernberger (F), Henry Sherman Oberly (H), Earl S. Rudisill (R), and Malcolm K. Macdonald (M). F and M were highly trained in giving introspective reports on psychophysical observations: H and R were less experienced in this sort of work. All had participated in the Training Series and were, therefore, thoroughly familiar with the apparatus and method of the experiment.

Handling of results. The judgments included, as stated above, quantitative estimates of subjective assurance, on a 5-point scale; following Oberly's procedure, the writer omitted from calculation all judgments given with an assurance less than '3,' *i.e.* an assurance so low that an *O* would not back it with at least an 'even-money' bet was assumed to be too much in the nature of a 'guess' to be significant. It is to be noted that F, M and H were aware of the omission of the '2' and '1' judgments in Oberly's experiment. F, however, was the only one who knew positively that a similar procedure was to be followed in this experiment. The suspicion that it might be so may conceivably have influenced H and M to give '3' judgments in some cases where the natural inclination would have suggested a '2.' Even if this were so, it seems highly improbable that any material difference would have resulted, inasmuch as all three intensities of stimulus would presumably have been affected equally. R had no knowledge of the omissions, being unacquainted with Oberly's paper.

The tables of observed relative frequencies were subjected to statistical treatment in accordance with the phi-gamma hypothesis. Limens (S) and coefficients of precision (h) were calculated for the two series separately. In this way a sort of check upon the results of the First Series was afforded by those of the Second Series, as to tendencies shown and effect of progressive practice.

Results

In Tables I to IV will be found the percentages of observed relative frequencies of correct judgments made by each of the four *Os*. The top line gives the systematic categories, Attention, Cognition and Apprehension, which we have applied to the three conscious patterns or 'methods of determining' the reports. In the second line appear the three stimulus intensities, Black, Dark and Light (Bk, D, and L) under each of the process patterns. The third line contains the three degrees of subjective assurance; 5, 4, and 3 (2 and 1 having been discarded as explained above) under each of the intensities.

In the first column are the values of the individual stimuli (2-12). In succeeding columns are to be found the percentages of correct judgments given for each stimulus value, with each degree of assurance, for all intensities, under each conscious pattern. The upper half of each table shows results for Series I, the lower half those for Series II.

In Tables V, VI, VII, and VIII, are indicated the calculated results for all the *Os*. The first column contains the systematic categories, Attention, Cognition, and Apprehension, each coupled with the three degrees of subjective assurance. The first line in each of the groups indicates the calculated results of the observed relative frequencies of correct judgments given with an assurance of '5' (100-1 bet); the second line shows the calculated results of correct judgments given with assurance '5' *plus* those with assurance '4;' the third line shows the calculated results of observed correct judgments given with all three degrees of assurance ('5,' '4,' and '3') in summation.

These calculated results are arranged in 6 columns: the first 3 containing the limens (S) for the different intensity-degrees (Bk, D, and L); the second 3 containing the values of h, or coefficient of precision, under the various categories. To facilitate comparison of results for the two series, both have been grouped under their respective categories.

Analysis of Results. In the formulation of our problem, it was assumed that if perceptual clearness bears some constant relation to intensity of stimulation, such relation would be reflected in the size of the limens and the shapes of the psychometric curves. In selecting stimuli we accordingly chose those which seemed of sufficient intensity-difference to produce such variation in the size of S and h, if the relationship is found to exist.

Determination of significant difference in size of these measures required the setting up of a standard. It was arbitrarily decided to adopt a difference of 0.5 as our criterion of signifi-

TABLE I
Observer F

Series	No. Stimuli	Category: Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	96	—	—	84	—	—	46	—	—	4	—	—	14	—	—	52	—	—	—	—	—	2	—	—	2	—	—
	3	82	—	—	48	—	—	40	—	2	18	—	—	50	—	—	52	4	—	—	—	—	2	—	—	—	2	—
	4	78	—	—	12	—	—	16	2	—	22	—	—	72	4	—	66	4	2	—	—	—	10	2	—	10	—	—
	5	34	—	—	4	—	—	8	2	—	50	6	—	44	2	—	30	4	—	4	6	—	14	24	8	10	26	14
	6	8	—	—	—	—	—	—	—	—	22	2	—	10	—	2	2	—	2	20	24	16	38	22	10	26	26	20
	7	—	—	—	—	—	—	—	—	—	4	—	—	—	—	—	—	—	—	10	18	34	16	24	22	4	14	36
	8	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	16	10	44	—	12	22	—	10	18
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2	34	—	2	12	—	—	8
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	18	—	—	4	—	—	6
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	2
Second	2	86	—	—	64	—	—	50	—	—	14	—	—	34	—	—	50	—	—	—	—	—	—	—	2	—	—	—
	3	68	—	—	40	—	—	38	—	—	28	—	—	58	—	—	60	2	—	4	—	—	2	—	—	—	—	—
	4	70	—	—	22	—	—	30	—	—	30	—	—	74	—	—	64	—	—	—	—	—	—	4	4	4	2	—
	5	50	—	2	12	—	—	6	—	—	40	—	—	48	2	—	52	—	—	4	2	—	22	14	2	14	14	10
	6	2	—	—	—	—	—	—	—	—	16	—	—	12	4	2	12	—	—	46	22	8	22	32	16	20	24	30
	7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	12	28	36	10	36	24	18	18	48
	8	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	10	24	34	—	8	34	6	—	38
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	4	40	—	2	32	—	—	30
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6	22	—	—	6	—	—	6
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	22	—	—	4	—	—	2
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	4	—	—	—

TABLE II
Observer H

Series	No. Stimuli	Category: Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	98	—	—	98	—	—	100	—	—	2	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—
	4	86	—	—	78	—	—	92	—	—	12	—	—	18	—	—	8	—	—	2	—	—	4	—	—	—	—	—
	5	22	—	—	24	—	—	16	2	—	54	2	8	64	4	—	64	2	4	14	—	—	8	—	—	6	—	4
	6	—	—	—	—	—	—	—	—	—	48	6	6	48	4	10	70	2	8	26	8	2	22	2	2	6	—	—
	7	—	—	—	—	—	—	—	—	—	26	6	2	24	10	4	22	4	16	22	14	12	26	4	14	18	8	12
	8	—	—	—	—	—	—	—	—	—	4	4	14	6	4	12	10	6	12	4	16	18	10	8	14	6	2	20
	9	—	—	—	—	—	—	—	—	—	2	6	4	4	—	4	—	2	4	4	2	10	—	—	10	4	2	4
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	4	—	—	6	—	—	2
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Second	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	100	—	—	96	—	—	98	—	—	—	—	—	4	—	—	2	—	—	—	—	—	—	—	—	—	—	—
	4	68	—	—	66	—	—	70	—	—	32	—	—	32	—	—	26	—	—	—	—	—	2	—	—	4	—	—
	5	18	—	—	12	—	—	24	—	—	76	—	—	84	2	2	70	—	2	4	—	—	—	—	—	2	—	2
	6	—	—	—	—	—	—	4	—	—	38	2	16	52	8	—	68	4	10	38	—	6	18	4	6	10	2	—
	7	—	—	—	—	—	—	—	—	—	46	6	4	50	4	4	30	2	10	18	8	4	28	2	6	22	6	22
	8	—	—	—	—	—	—	—	—	—	18	2	8	14	—	10	18	4	18	24	4	14	28	8	28	18	—	22
	9	—	—	—	—	—	—	—	—	—	6	2	6	6	—	6	2	—	6	14	2	28	2	—	14	16	6	16
	10	—	—	—	—	—	—	—	—	—	4	—	2	2	—	—	—	—	4	8	—	20	2	—	8	4	—	6
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	—	10	2	2	4	—	—	4
	12	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

TABLE III
Observer R

Series	No. Stimuli	Category																								
		Attention									Cognition								Apprehension							
		Bk			D			L			Bk			D			L		Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	38	—	—	52	—	—	46	—	—	62	—	—	48	—	—	54	—	—	—	—	—	—	—	—	—	—	—
	4	24	—	—	12	—	—	26	—	—	56	6	—	52	4	—	34	4	—	12	—	—	26	4	—	22	10	4
	5	6	—	—	6	—	—	2	—	—	62	4	—	66	12	—	66	6	—	4	8	4	2	2	2	—	6	4
	6	—	—	—	—	—	—	2	—	—	18	6	—	20	10	—	20	10	—	4	12	—	10	20	6	6	22	2
	7	—	—	—	—	—	—	—	—	—	4	—	—	6	4	—	6	—	—	—	24	10	4	24	10	4	20	10
	8	—	—	—	—	—	—	—	—	—	—	2	—	—	2	—	—	—	—	2	8	2	—	10	10	—	6	12
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	10	—	—	2	—	—	—	6
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	4	—	6	—
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	4	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	2	—	—	—	—	—	2
Second	2	100	—	—	100	—	—	100	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	3	86	—	—	82	—	—	86	—	—	14	—	—	18	—	—	14	—	—	—	—	—	—	—	—	—	—	—
	4	80	—	—	56	—	—	64	—	—	16	—	—	34	—	—	28	—	—	4	—	—	8	—	2	8	—	—
	5	18	—	—	24	—	—	4	—	—	70	—	—	32	2	—	74	2	2	2	—	—	2	4	—	6	—	—
	6	—	—	—	—	—	—	—	—	—	18	—	2	30	8	—	50	2	2	20	24	6	22	4	—	6	12	2
	7	—	—	—	—	—	—	—	—	—	2	2	—	8	—	—	8	—	—	8	22	8	18	28	4	10	24	2
	8	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	—	—	—	2	20	6	2	14	12	2	10	8
	9	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	6	6	—	—	8	—	2	4
	10	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	6	—	—	—
	11	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	8	—	—	—	—	—	—
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	—	—	—	4

TABLE IV
Observer M

Series	No. Stimuli	Category: Attention									Cognition									Apprehension								
		Bk			D			L			Bk			D			L			Bk			D			L		
		5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3	5	4	3
First	2	74	—	—	94	—	—	82	—	—	22	—	—	4	—	—	18	—	—	4	—	—	2	—	—	—	—	—
	3	98	—	—	82	—	—	90	—	—	2	—	—	16	—	—	8	—	—	—	—	—	—	—	—	2	—	—
	4	18	—	—	2	—	—	8	—	—	80	—	—	94	2	—	90	—	2	—	—	—	2	—	—	—	—	—
	5	—	—	—	—	—	—	2	—	—	94	—	—	96	—	—	96	2	—	2	—	—	2	—	—	—	—	—
	6	—	—	—	—	—	—	—	—	—	70	2	2	84	6	2	86	4	—	22	—	—	2	—	—	6	2	—
	7	—	—	—	—	—	—	—	—	—	48	14	8	62	26	2	56	16	2	4	2	—	4	2	—	2	4	—
	8	—	—	—	—	—	—	—	—	—	26	22	12	12	26	6	16	32	6	4	4	2	4	4	—	—	—	—
	9	—	—	—	—	—	—	—	—	—	6	42	12	—	10	16	—	18	16	—	2	4	—	—	—	—	4	—
	10	—	—	—	—	—	—	—	—	—	—	16	22	—	4	6	—	4	18	—	4	—	—	—	2	—	2	6
	11	—	—	—	—	—	—	—	—	—	—	—	10	—	—	4	—	—	—	—	—	2	—	—	2	—	—	2
	12	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Second	2	52	—	—	76	—	—	56	—	—	48	—	—	24	—	—	44	—	—	—	—	—	—	—	—	—	—	—
	3	66	—	—	58	—	—	58	—	—	34	—	—	42	—	—	42	—	—	—	—	—	—	—	—	—	—	—
	4	4	—	—	4	—	—	—	—	—	96	—	—	96	—	—	100	—	—	—	—	—	—	—	—	—	—	—
	5	8	—	—	—	—	—	6	—	—	90	2	—	100	—	—	92	—	—	—	—	—	—	—	—	—	—	—
	6	—	—	—	—	—	—	—	—	—	100	—	—	88	4	—	96	2	—	—	—	—	2	—	—	2	—	—
	7	—	—	—	—	—	—	—	—	—	76	8	—	82	10	2	78	10	4	6	2	—	2	—	—	2	2	—
	8	—	—	—	—	—	—	—	—	—	66	24	2	36	30	10	56	24	12	—	—	—	—	2	2	—	—	—
	9	—	—	—	—	—	—	—	—	—	14	26	10	12	26	6	12	24	14	—	14	2	—	6	—	2	4	4
	10	—	—	—	—	—	—	—	—	—	8	18	18	—	6	14	2	18	8	—	10	2	—	4	8	—	12	2
	11	—	—	—	—	—	—	—	—	—	—	6	6	—	6	14	—	2	8	—	6	12	—	6	14	—	—	12
	12	—	—	—	—	—	—	—	—	—	—	—	2	—	—	4	—	—	—	—	—	2	—	—	6	—	—	6

cance for the limens and one of 0.2 for the precisions. Differences less than these we believed would be non-significant or, at best, of doubtful significance.

Turning to examination of the calculated results, Tables V to VIII, we find progressive increase in the size of the limens for all *O*s, from Attention to Cognition, to Apprehension. This was, of course, to be expected, inasmuch as the categories are progressively inclusive, as stated above. The improvement incident to progressive practice is evident by comparison of limens for Series II with those of Series I.

TABLE V
Observer F

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	4.479	2.961	2.015	0.541	0.686	0.321
		5+4	4.479	2.961	1.975	0.541	0.686	0.293
		5+4+3	4.479	2.961	2.033	0.541	0.686	0.296
	Second	5	4.496	2.600	2.833	0.324	0.367	0.436
		5+4	4.496	2.600	2.833	0.324	0.367	0.436
		5+4+3	4.463	2.600	2.833	0.335	0.367	0.436
Cognition	First	5	5.677	4.874	4.619	0.995	0.661	0.671
		5+4	5.784	4.930	4.785	1.092	0.667	0.695
		5+4+3	5.784	4.960	4.809	1.092	0.657	0.725
	Second	5	5.502	5.118	5.110	0.622	0.656	0.841
		5+4	5.502	5.200	5.143	0.622	0.628	0.967
		5+4+3	5.525	5.236	5.143	0.616	0.617	0.967
Apprehension	First	5	6.169	5.726	5.117	0.518	0.531	0.566
		5+4	6.673	6.597	6.089	0.447	0.567	0.537
		5+4+3	8.443	7.420	7.192	0.359	0.486	0.405
	Second	5	6.396	5.661	5.650	0.588	0.561	0.592
		5+4	6.972	6.587	6.216	0.426	0.468	0.609
		5+4+3	8.956	7.937	7.871	0.341	0.378	0.442

For H and M improvement is most marked in Cognition and Apprehension, while in Attention there is a decrease, which is probably due to a tendency to characterize as *Grouped* some judgments originally given under the category of *Immediate*, rather than to an actual decrease of ability to grasp the stimuli immediately.

For F there is slight improvement Attention, especially for L stimuli, less for Bk and none for D. Under Cognition, F shows no improvement for Bk, slight for D and more marked for L. Under Apprehension, F shows uniform improvement (with one slight inversion), the greatest being again in L.

R shows marked and steady improvement throughout; he stated in one of his introspections, "there were more *Immediate* judgments to-day, for I seemed more capable of grasping an entire field. Re-imaging larger groups of dots was also easier."

Limens compared for the several Os. A comparison of the limens of Attention, Cognition, and Apprehension for the different stimuli and the different degrees of assurance reveals the following:

H, Table VI. The data show that in Series I, Attention, assurance 5, the order of the limens are L highest, Bk intermediate, D lowest; the differences, however, fall far below our criterion of significance; L being 0.029 greater than B., and 0.069 greater than D; Bk 0.04 greater than D. With assurance 5 + 4 and 5 + 4 + 3 the same order is maintained, the only variation being an increase of 0.018 for the L limen, a difference

TABLE VI
Observer H

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	4.556	4.516	4.585	1.091	1.001	1.710
		5+4	4.556	4.516	4.603	1.091	1.001	1.623
		5+4+3	4.556	4.516	4.603	1.091	1.001	1.623
	Second	5	4.338	4.240	4.468	0.966	1.048	0.870
		5+4	4.338	4.240	4.468	0.966	1.048	0.870
		5+4+3	4.338	4.240	4.468	0.966	1.048	0.870
Cognition	First	5	6.032	6.149	6.290	0.549	0.525	0.550
		5+4	6.140	6.353	6.825	0.414	0.505	0.485
		5+4+3	6.642	6.675	6.926	0.418	0.443	0.479
	Second	5	6.418	6.668	6.613	0.392	0.488	0.552
		5+4	6.552	6.796	6.714	0.384	0.506	0.535
		5+4+3	6.966	6.905	7.218	0.376	0.419	0.459
Apprehension	First	5	6.787	6.854	6.687	0.523	0.583	0.522
		5+4	7.255	7.173	7.005	0.424	0.543	0.488
		5+4+3	8.224	7.957	7.863	0.567	0.519	0.526
	Second	5	7.626	7.463	7.379	0.381	0.450	0.454
		5+4	7.906	8.204	7.632	0.391	0.480	0.459
		5+4+3	9.118	8.802	8.832	0.379	0.577	0.573

which is still much below what we decided to consider significant. In Series II, Attention, similar relations are found as in Series I, only exaggerated; here the limens are the same for all three degrees of assurance, which indicates that judgments were given with assurance 5 only. L is 0.130 higher than Bk, and 0.228 higher than D; while Bk is 0.098 higher than D. No significance.

The results for Cognition, Series I, Assurance 5 show complete inversion, L being highest, D intermediate and Bk lowest; the differences being: L 0.141 greater than D, and 0.258 greater than Bk; D 0.117 greater than Bk. No significance.

With Assurance 5 + 4, similar though greater differences obtain; L is 0.472 greater than D and 0.685 greater than Bk. Here is our first occurrence of a significant difference, but it is reversed, *i.e.* the limen for the least intensive stimulus is higher than that for the most intensive!

With Assurance 5 + 4 + 3, the order is the same, viz. L, D, Bk; but the differences are non-significant, due to a greater relative increase in the size of Bk than for either D or L, and greater for D than for L. Under Cognition, Series II, Assurance 5 and 5 + 4, there is a shift in the order to D, L, Bk; with Assurance 5 + 4 + 3 another shift to L, Bk, D, but with no significance in any case.

Passing to the limens for Apprehension, Series I, Assurance 5, we find another shift to D, Bk, L, and again a change in the order for Assurance 5 + 4 and 5 + 4 + 3, to Bk, D, L; still without significant difference. In Series II, Assurance 5, the relative order remains the same, viz. Bk, D, L; not significantly different. With Assurance 5 + 4, a shift to D, Bk, L, occurs with differences as follows: D 0.0298 greater than Bk, D 0.572 greater than L, and Bk 0.278 greater than L. Here is a significant difference as between D and L, the first and only time for this *O* when a more intense stimulus produced a significantly higher limen than that for the less intense!

TABLE VII

Observer R

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	2.576	2.966	2.917	0.418	0.611	0.509
		5+4	2.576	2.966	2.917	0.418	0.611	0.509
		5+4+3	2.576	2.966	2.917	0.418	0.611	0.509
	Second	5	4.352	4.148	4.030	0.719	0.576	0.906
		5+4	4.352	4.148	4.030	0.719	0.576	0.906
		5+4+3	4.352	4.148	4.030	0.719	0.576	0.906
Cognition	First	5	5.157	5.000	4.999	0.639	0.474	0.376
		5+4	5.358	5.269	5.160	0.638	0.457	0.425
		5+4+3	5.358	5.269	5.190	0.638	0.457	0.355
	Second	5	5.592	5.613	5.790	0.832	0.696	0.636
		5+4	5.610	5.705	5.808	0.814	0.673	0.656
		5+4+3	5.622	5.705	5.866	0.812	0.673	0.642
Apprehension	First	5	5.431	5.540	5.358	0.699	0.640	0.543
		5+4	6.210	6.455	6.378	0.353	0.529	0.413
		5+4+3	6.399	6.799	6.595	0.304	0.452	0.339
	Second	5	5.927	6.196	6.074	0.741	0.679	0.703
		5+4	6.634	6.815	6.585	0.461	0.522	0.545
		5+4+3	6.999	7.050	6.901	0.330	0.423	0.346

It is to be noted, at this point, that from a quantitative point of view there is less intensity-difference between the two grays than between the dark gray and the black stimulus, as is shown by the relative percentages of black noted under *Apparatus*. With Assurance 5 + 4 + 3, the order again changes to Bk, L, D, the differences being non-significant.

Summing up the results for Observer H, we find that there are but two cases where a significant difference is shown between limens for different intensities of stimulus under the same process and with the same degree of assurance. In Series I, Cognition, Assurance 5 + 4, the L limen was 0.685

higher than the Bk. In Series II, Apprehension, Assurance 5 + 4, D gave a limen 0.572 higher than did L. In 10 cases L was highest; Bk and D were highest in 4 cases each.

R, Table VII. Without going into so minute detail in examination of R's limens, we may summarize the results as follows:

The relative order under Attention, Series I, all assurances, (judgments under this category being given with assurance 5 only) is: D, L, Bk. For Series II, it is Bk, D, L, with non-significant differences. Under Cognition, Series I, all assurances, the order is Bk, D, L; Series II, all assurances, L, D, Bk. Under Apprehension, both series, all assurances, the D limens are high in every case; intermediate values are given for Bk in 3 cases and for L in 3 cases.

In no case was there a significant difference for R. In 9 cases D was highest, in 6 cases Bk, and in 3 cases L.

TABLE VIII

Observer M

Category	Series	Assurance	Limen			precision (h)		
			Bk	D	L	Bk	D	L
Attention	First	5	3.124	3.277	3.211	0.542	1.113	0.766
		5+4	3.124	3.277	3.211	0.542	1.113	0.766
		5+4+3	3.124	3.277	3.211	0.542	1.113	0.766
	Second	5	2.607	2.863	2.710	0.411	0.739	0.390
		5+4	2.607	2.863	2.710	0.411	0.739	0.390
		5+4+3	2.607	2.863	2.710	0.411	0.739	0.390
Cognition	First	5	6.848	7.148	7.081	0.373	0.396	0.496
		5+4	8.038	7.619	7.716	0.254	0.395	0.417
		5+4+3	8.737	7.960	8.308	0.253	0.361	0.361
	Second	5	8.062	7.672	7.899	0.529	0.599	0.615
		5+4	8.903	8.493	8.708	0.475	0.491	0.499
		5+4+3	9.275	8.994	9.164	0.417	0.392	0.456
Apprehension	First	5	7.277	7.228	7.138	0.446	0.481	0.627
		5+4	8.489	7.804	7.948	0.338	0.475	0.489
		5+4+3	9.237	8.207	8.534	0.304	0.409	0.407
	Second	5	8.134	7.714	7.951	0.553	0.642	0.625
		5+4	9.355	8.770	8.966	0.481	0.456	0.457
		5+4+3	9.908	9.579	9.636	0.435	0.329	0.413

M, Table VIII. M's results show the following features: In Attention, both series, all assurances, the D limens are the highest, the intermediate values going to L. The differences are all small, however, the greatest being that between D and Bk, Series II, D being 0.256 higher than Bk. In Cognition, we find the order Bk, L, D in every case but one, where it is D, L, Bk (Assurance 5, Series I); with Assurance 5 + 4 + 3, Series I, occurs the first significant difference: Bk 0.777 greater than D, but L 0.348 greater than D.

In Apprehension, both series, all assurances, the order is the same as for Cognition, viz. Bk, L, D, in every case but one, where it is Bk, D, L (Assurance 5, Series I).

In 5 cases we find significant differences, always in favor of the Bk: 3 times against D, and twice against L.

Thus we find M showing a significant difference 6 times: none under Attention, once under Cognition, 5 times under Apprehension, and always in favor of Bk.

Considering absolute differences, we find: Bk highest 11 times, D highest 7 times, L highest no times, Bk intermediate no times, D 1 time, L 17 times, Bk lowest 7 times, D 10 times, L once.

F, Table V. In F's results we find a striking contrast to those of the other *Os*. In every case the Bk limens are the highest; in 15 cases the D limens are greater than the L, but in only 3 cases are the limens higher than D. Significant differences are shown between Bk and L, in every case except Cognition, Series II, all assurances; Bk, is significantly higher than D in every case in Attention, both series; in every case in Cognition, Series 1. In 3 cases under Apprehension, D is significantly higher than L; also in Attention, Series I, all assurances; and in Apprehension, Series I, Assurances 5 and 5 + 4. Bk highest 18 times, D no times, L no times; D higher than L 15 times; L higher than D 3 times.

Introspections. Additional evidence of the inconstancy of effect of intensity-differences upon the size of the limens is afforded by some spontaneous introspective reports of the *Os*.

F, who, it will be remembered, knew in advance of the intensity-differences, stated at the close of the first observation period that "in not over 6 of the cards was he conscious of any differences in brightness of the dots."

H reported after his first observation period, "there are, I believe, several kinds of cards with dots of black and gray. On not more than three cards was I conscious of the difference. One card seemed to 'blare,' because of the great number of black dots." Three weeks later he reported, "Eight cards definitely gray, three definitely black." And, again, one week later, "I noticed that there were cards with black dots that were almost impossible for me. They seemed to be too bright, while the gray dots were not nearly so hard to attempt. I do not think that there were more than 10 cards exposed today in which I was conscious of the difference in color of the dots." Two months later, he said: "A very few times I was conscious of difference of brightness; some cards with black dots were very vivid."

R gave, at the very last, in the course of a detailed introspection on every card of a whole series, the following varied reports for the different intensities:

Bk stimuli. "They stood out boldly" [the number apprehended was incorrect]. "Clear at first, but I couldn't grasp any on presentation (of report)" [Number wrong]. "Clear—re-imaged," [Number wrong].

D stimuli. "The dots seemed very bright—the clearness was quite pronounced" [Number wrong]. "Very clear" [Number correct]. "Not so clear, but certain at once" [Number correct]. "Fairly clear—no uncertainty" [Number correct].

L stimuli. "The whole thing faint" [Number correct]. "Not very clear" [Number wrong]. "Not so clear—absolute certainty—came at once—no re-imaging required—dots seemed large but not saturated" [Number correct].

The introspections of R are strongly confirmatory of Dallenbach's finding relative to the independent variability of attributive and cognitive clearness. With attributive clearness high or low may be combined either high or low cognitive clearness.[18]

[18]Dallenbach, Attributive *vs.* cognitive clearness, *loc. cit.*

The introspective reports, quoted above, of F, H, and R, and others less specific of M, all indicate clearly the extreme variability of the intensity effect.

Size of h *compared for the several Os.* Observation of the size of the coefficients of precision (h), for the 3 categories for the several *Os*, shows the following features.

For H, the values of h are highest for Attention, for all intensities and all degrees of assurance; there is about equal division of intermediate values between Cognition and Apprehension, with no definite trend toward any one intensity.

For F, the h-values are highest for Cognition with Bk and L; for Cognition and Attention with D.

For R, Cognition shows the higher h-values for Bk and D, in Series II; and for Bk in Series I, whereas Attention gives the higher values of h for L in Series II, and for D and L in Series I.

For M, Attention gives higher values for h for Bk, D, and L in Series I, and for D, in Series II. Apprehension gives higher values for Bk and L in Series II.

The significant differences—the criterion of 0.2, it will be remembered, was fixed upon as suitable—are, in the main, as indicated above. In several cases, in addition to the foregoing, significant differences are shown between the value of h for assurances 5 and 5 + 4 + 3, but without altering the ratios.

No constant trend whatever appears in these h values, in relation to either differences of intensity or conscious process. These results confirm Fernberger's findings in his study of the range of visual apprehension in which he states, "it was expected that the form of the curves might vary for the different series, thus showing the influence of the brightness of the backgrounds" [he used black dots on a white and on a medium gray background] . . . but "no such variation is present for any subject."[19]

Discussion

Table IX summarizes the relative frequencies with which the possible orders of precedence as to size of limens occurred for the several *Os*. For instance, the number of times that the limens occurred in the order, Bk highest, D intermediate, L lowest was for F, 15 times; for H, 3 times; for R, 6 times; for M, once. At the other extreme, the order L high, D middle, Bk low, occurred only 6 times: 3 times each for H and for R.

By using different combinations, we may arrive at determinations of various relative frequencies of occurrence. For example, we find by combining the figures lying in the first and second rows, in any vertical column, the number of times the Bk stimulus gave the highest limen, *e.g.* for F, 18 times; for H

[19]Fernberger, *op. cit.*, 129.

4 times; etc. Similarly, the number of times L produced a higher limen than Bk, for all *O*s, may be found by adding the totals of rows 4, 5, and 6.

From Table IX it will be seen that for F, Bk was by far the most effective stimulus, giving the limens higher than both the grays in every case; D was next in order of effectiveness, in that its limens exceeded those of L in 15 out of 18 times.

TABLE IX

RELATIVE FREQUENCIES OF DIFFERENT ORDERS OF LIMENS

Order of Limens	*O*				Totals
	F	H	R	M	
Bk—D—L	15	3	6	1	25
Bk—L—D	3	1	0	10	14
D—Bk—L	0	2	3	0	5
D—L—Bk	0	2	6	7	15
L—Bk—D	0	7	0	0	7
L—D—Bk	0	3	3	0	6

For M, too, Bk was the most effective, as shown by its frequency of predominance: the Bk limen was higher than both D and L, 11 times. On the other hand, L limens exceeded Bk 7 times, and D 10 times, indicating intermediate effectiveness of L, instead of D, as shown in F's results.

For R, D appears the most effective of the 3 intensities, exceeding Bk 12 times, and L 15 times. Bk is next most effective, and L least.

For H, L shows higher than Bk 12 times, and higher than D 11 times. Bk comes next with 11 limens higher than the D, and 6 higher than L.

From the foregoing it appears that intensity does not exercise a constant effect upon the size of the limens. For F, the order of effectiveness is Bk, D, L; for H: L, Bk, D; for R: D, Bk, L; and for M: Bk, L, D.

TABLE X

RELATION OF THE LIMENS

Category	Relation	*O*				Relation	*O*			
		F	H	R	M		F	H	R	M
Attention	L — Bk	0	6	3	6	Bk — L	6	0	3	0
	D — Bk	0	0	3	6	Bk — D	6	6	3	0
	L — D	3	6	0	0	D — L	3	0	6	6
Cognition	L — Bk	0	6	3	1	Bk — L	6	0	3	5
	D — Bk	0	5	3	1	Bk — D	6	1	3	5
	L — D	0	3	3	5	D — L	6	3	3	1
Apprehension	L — Bk	0	0	3	0	Bk — L	6	6	3	6
	D — Bk	0	2	6	0	Bk — D	6	4	0	6
	L — D	0	2	0	5	D — L	6	4	6	1

Referring to Table X, we find the relative frequencies of predominance arranged under the systematic categories or conscious processes.

In the first 4 columns appear the number of times, for each *O*, that a less intense stimulus gave a higher limen than did a

more intense, while in the second 4 columns the reverse is shown. Each group is fractionated into the 3 systematic categories or conscious processes, under which the frequencies occurred.

For F, only under the category of Attention, there appeared a predominance for a less intense stimulus: L was greater than D 3 times. In every other case the more intense stimulus always gave higher limens.

For H, a predominance of L is shown, over both Bk and D, every time under Attention, and a similar predominance of Bk over D. Under Cognition, the same relation holds for L as compared with Bk; D, however, takes the lead over Bk in 5 cases out of 6; while the L predominance over D drops to 50% (3 out of 6). Passing to Apprehension, we find the superiority definitely shifted to the more intense stimuli: L going below Bk every time; D below Bk 4 out of 6 times, and L below D 4 times.

For R, the predominant effectiveness of D as conditioning higher limens is definitely shown in passing from Attention to Apprehension.

For M, the tendencies noted earlier are here confirmed under Cognition and Apprehension, viz. Bk greater than D, Bk greater than L, and L greater than D.

From these data it seems safe to generalize the relations as follows: In general under the conditions of this experiment, objective intensity of stimulation is most effective in conditioning perceptual clearness under the process of Apprehension, less so under Cognition, and least under Attention.

Let us now scrutinize more closely the conscious processes here involved, in an attempt to get some further light on the differences shown above.

It is obvious that in passing from Attention, to Cognition, to Apprehension, we are traversing an ascending scale of mental complexity. Attention is the simplest process; in it the stimulus material comes to consciousness in a single flash of awareness, with an equal and high degree of clarity. In regard to this category, the introspective reports of the *Os* agree.

R reported: "When I characterized a judgment as *Immediate*, it was immediate, no intervening process—all happened in an instant and I was certain at once." Again: "Equal clarity—all in an instant—no re-imaging—absolute certainty and all came at once—no verification was needed."

H reported: "The *Immediate* judgments are based on the grasping of the dots without any conscious grouping or counting." "*Immediate* reports were clear—no question in my mind, and no subsequent process was involved."

"*Immediate* reports were given when all dots were clear—a focus which left no doubt in my mind."

M reported: "By *Immediate* I mean that all dots are clear and are perceived immediately upon the fall of the shutter."

It seems evident that this category is correctly described. In Cognition, which we defined tentatively as one in which "immediate perception occurred, but owing to different degrees of clearness, there is an attentional grouping of the stimulus dots," all *Os* agree as to the process of becoming aware of groups of dots.

H states: "In *Grouping* there is a definite grouping of the dots, generally accomplished by taking in twos, threes, or fours."

R says: "When I answer *Grouped*, several dots 'stick together' forming a unit, while another or several other units are formed at other locations."

M reports: "The dots seem more or less naturally to fall into groups. If there is merely one of these groups I report an *Immediate* judgment; if two or more groups I report *Grouping*."

So far as the grouping is concerned, there is no question, but when the factor of immediacy of the *Grouped* judgments is scrutinized, serious doubts arise.

R reported: "The *Grouped* judgments are almost immediate, with some one group particularly clear, the others less clear, but all somehow belonging together." "Got 3 groups of 2 each—it seemed the middle 2 first, then 2 above and then 2 below—no re-imaging—but there seemed to be temporal succession" (this report was in response to a card with 6 L dots). "Three and three, got those to right slightly ahead of those to left" (this on another 6-stimulus card, D dots).

H reported: *Grouped* judgments as based on a reading of the after-image, *i.e.* the *Anhang* or perseverative image, subsequent to actual stimulation.

M reported: *Grouped* judgments as based on a 'successive' awareness of the dots, *e.g.* a '2' judgment was "one dot, then another;" '3' was "two and one —succession."

These introspective reports from three of the *O*s definitely indicate a temporal sequence as characterizing this process, yet without the mediation of re-imaging. The actual retinal stimulation gives rise to an immediate awareness in which all the dots are of equal and high degree of clearness; the judgment based on this momentary flash of consciousness is what we have called Attention. The conscious effect of stimulation is not, however, confined to this momentary flash, but continues for a more or less durative period as the perseverative image. It seems that the *Grouped* judgment (Cognition) is arrived at by a reading of this image, by 'successive attentions'[20] to smaller groups, and a rapid totaling of these part-judgments.

The *Counted* judgments, or Apprehension as we have called this process, seem definitely to be characterized by a re-imaging or calling back into consciousness of a process which has, wholly or in part 'faded out.' Frequently, if not always, some group or groups remain in consciousness, as part of the perseverative image which has not 'faded;' this forms the nucleus which is later amplified by the re-imaged dots.

H said: "With *counting* I use any method, mixing up *grouping* and *counting*. I generally have an immediate flash of the dots at the point of fixation and begin to group or count from that point."

R states: "With large numbers in *counted* judgments, I usually get a part of the field immediately, usually the upper left."[21]

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[20]Pillsbury, *Attention*, 1908, 67.

[21]It is interesting to note that the three *O*s who mentioned the direction in which the image was read were all different in this respect: R read the dots from left to right; M from top to bottom; and H from bottom upward.

Thus, it appears, as Oberly observed in his paper,[22] that progressive elaboration of mental process occurs as the complexity of the stimulus material increases, *i.e.* as the number of dots is increased.

Reference to Tables I to IV shows the relative frequencies of individual judgments for the several *O*s in respect to this elaboration of process.

F, Table I. F shows a gradual shift from Attention to Cognition. The great majority of his Bk 2-, 3-, and 4-dot cards, and most of the D 2- and 3-dot cards were grasped under the process of Attention; the L stimuli being for him less effective, required an earlier shift to Cognition. The shift from Cognition to Apprehension, occurs rather suddenly at the 5-dot cards for D and L, and at the 6-dot cards for Bk. F depended upon the more complex process of Apprehension for the grasping of everything above 6, with the exception of four Bk 7-dot cards.

H, Table II. With H the shift from Attention to Cognition occurs rather suddenly at 5, and that from Cognition to Apprehension at 6 and 7. This *O* shows an almost equal distribution of Cognition and Apprehension judgments.

R, Table III. With R, the break from Attention to Cognition occurs at 3, and a rather high percentage of 6 come under Apprehension. The 2nd Series shows the great improvement made by this *O* as a result of progressive practice.

M, Table IV. M's results show an almost complete dependence upon Cognition. It seems a rather significant fact that this *O* was of strongly visual concrete imaginal type. This fits in well with Hylan's findings that length of span was correlated with relative duration of visual after-image.[23] This *O* showed the highest limens of all the *O*s for both Cognition and Apprehension. Inspection of his results for Series II shows also the very high assurance with which his judgments were given: only 8 had assurance less than 5 up to and including the 6-stimuli.

Thus it would seem that for the lower values of stimulus, the univocal process of Attention is adequate, and the awareness comes immediately and with maximal clearness. With the intermediate stimulus-values a more complex conscious process is required, which takes, sometimes at any rate, a certain length of time for exploration of the after-image. With the higher values, the organism is pushed to its utmost to grasp the material presented, and has recourse to whatever additional means are at its disposal, viz. a re-imaging of the material. If this analysis is correct, the reason is plain why intensity differences fail to become operative in the majority of cases until the more complex processes are brought into play. The experimental setting and the process of training bring about the establishment of a mental 'set' that is of varying efficacy for the different *O*s.

[22] Oberly, *op. cit.*
[23] J. P. Hylan, The distribution of attention, *Psychol. Rev.*, 10, 1903, 373-403, 498-533.

For instance, with H, the experimental conditions were such that it was not until the higher stimulus values were presented that he was forced to avail himself of every factor in the situation that could serve to bring the awareness for which he was striving. Under the process of Apprehension, with its expedient of re-imaging added to the operations used in Attention and Cognition, the factor of intensity at length became operative and gave the correspondingly higher limens for Bk and negligible differences between the D and L, due to too slight intensity differences.

With F, greater experience in introspective work along the lines of psychophysical observation enabled him to avail himself of the intensity-differences at an earlier stage.

R's predominance of D over Bk, and the equality of intensity effect as between Bk and L in Apprehension, offer an anomalous situation, as do also, though in less degree, M's higher limens for L than for D.

Pending further illumination on this perplexing question, we may be justified in formulating, in a tentative way, the conclusion that:

Intensity differences are of variable effectiveness in conditioning clearness of perception, the latter being presumably reflected in the size of the limens as shown in this experiment.

Summary

(1) Liminal differences are shown for all three values of stimulus-intensity.

(2) Stimulus intensities do not, however, seem to have any constant effect for any of the *O*s.

(3) The stimuli effective in conditioning highest limens are: for F, Black definitely the most effective; for M, Black most effective, with Light predominant over Dark; for R, Dark; for H, Light.

(4) In general, a predominant effectiveness is found for the more intense stimuli under the category of Apprehension, less so under Cognition, and least under Attention.

(5) Confirming Fernberger's findings, no constant variation is shown by the values of the coefficients of precision as reflecting the influence of variations of stimulus-intensity.

THE RANGE OF VISUAL ATTENTION, COGNITION, AND APPREHENSION FOR COLORED STIMULI

By JOHN R. HART, Jr., University of Pennsylvania

Recent experiments on the 'Range of Attention' problem have made it evident that the usual experimental procedure and instructions give results for a mental process much more complicated than could possibly be included within the term Attention.[1] In order to further study this difficulty, Oberly,[2] on the basis of preliminary introspective reports, calculated the statistical limens for three typical processes which he called attention, cognition, and apprehension.

Oberly employed black stimuli on a white background exposed tachistoscopically. He found that each of his three systematic categories gave characteristically different thresholds: those for attention being smallest, cognition next, and apprehension largest. In the present study, Oberly's systematic analysis of the problem has been accepted and an attempt is made to determine the effect of color upon the statistical limens for each of Oberly's categories.

EXPERIMENTAL PROCEDURE

The introspective reports of the *Os* indicate that there are possible, and of very frequent occurrence, 3 conscious patterns which form the basis of the verbal report of the stimuli perceived. These 3 patterns were described as follows: (1) *Immediate.* All the dots were perceived in a single flash of consciousness, with an equal and high degree of clearness. (2) *Grouping.* All the dots were seen in a single flash of consciousness, but owing to different degrees of clearness, there was an attentional grouping of the stimulus dots. Both of the 2 conscious patterns characterized above, however, gave rise at once to the verbal report without any mediating conscious processes. (3) *Counting.* This process was different from either of the other two, inasmuch as the report was always made on the basis of some subsequent re-imaging of the stimulus material. The mediating process varied from *O* to *O* and for the same *O* from time to time. The processes consisted frequently in visual images, the dots of which were then counted verbally; or of a verbal counting with ocular-motor fixation of the position of the dots or other similar sorts of processes. Also, all of the *Os* reported wide differences in the degree of subjective assurance with which the judgments were reported.

Apparatus. Three sets of gray cards 87 mm. square were prepared. From 2 to 12 round dots of each color (6.5 mm. in diam.): blue, green, yel-

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[1] Cf. S. W. Fernberger, A preliminary study of the range of visual apprehension, this JOURNAL, 32, 1921, 121-133.
K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230.

[2] H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-352.

low or red of the Hering papers in each set, were pasted in haphazard arrangements on the gray backgrounds. Each set was homogeneous. All of these were exposed by means of a Whipple tachistoscope with an exposure of 40 sigma. There were 44 cards in each set, making 132 in all. The order of the stimulus cards was different in each of the three sets and was arranged in haphazard fashion. After the exposure of all the cards, they were turned 90° so that the right edge of each card was now the bottom of the card for the next group of exposures. This method of rotating the cards was continued throughout the experiment. Thus any stimulus card was seen in the same (identical) position only after each 528th exposure. In this manner and also by starting at the different ends of each series the probability of the *O*'s memorizing the order of the series or of his recognizing any particular card was very much reduced.

The photographic dark room of the laboratory was used in order to keep the illumination on the stimulus card and the general illumination constant, the only source of light being that of the exposure lamp. The *O* was seated 1.9 m. from the pre-fixation screen. Each *O* gave 2,200 judgments for each fractionated series, making 4,400 total, or 400 judgments for each number of dots, 100 judgments for each color of each number of dots.

Instructions. The directions which were read to the *O* at the beginning of each experimental sitting were as follows: "You will be shown successively, for a very brief exposure, cards which contain a varying number of dots. Look at these and report as follows.

"(1) The number of dots you have seen.

"(2) The degree of assurance or certainty of your judgment, using the following scale for your report: 5—if it is a 100 to 1 bet, 4—if it is a 5 to 1 bet, 3—if it is a 1 to 1 bet, 2—if it is a 1 to 5 bet, 1—guess or do not know.

"(3) An introspective characterization as follows: (1) *Immediate*, which means that the dots were perceived in a single flash of attention and were ungrouped. (2) *Grouping*, which means that the dots were seen in a single flash of attention but were grouped. (3) *Counting*, which means that the dots were re-imaged in some fashion, subsequent to the exposure, other than for verification, and were subsequently counted."

Each *O* was given some practice in following these directions before the record was taken of his judgments.

It is obvious that the introspective characterizations do not correspond to the systematic categories that have appeared in the psychological literature.[2] It was necessary therefore to transform the results obtained into some form of systematic categories for the purpose of study and of calculation. From a study of the literature it seems that the introspective characterization which has been described above and which appears in the instructions as 'immediate' corresponds to one-level attention. These results have therefore been calculated under this heading.

The term 'cognition' as it appears in the psychological literature seems to be a process which is immediate but which does not imply only a single level of clearness. The principal aspect which characterizes cognition is its immediacy. It will be observed that the introspective categories of *Immediate* and *Grouping* are both characterized by their immediacy, although the second is further characterized by different degrees of clearness which lead to an attentional grouping. The results of these two introspective categories have therefore been combined under the title of cognition.

The term 'apprehension' as it appears in the psychological literature signifies the obtaining of a knowledge of the stimulus without restriction of the processes underlying such knowledge. All three of the introspective categories have therefore been combined for purposes of calculation for

[2]This argument follows Oberly's analysis noted above.

this systematic concept. In other words, the results of both *Immediate* and *Grouping*, as the two ways of getting knowledge without subsequent process, have been combined for *Counting*, which involves such a mediating process.

The results for Attention consist solely of those which were characterized as immediate and ungrouped by the *Os*. The results for Cognition are those which the *Os* characterized as immediate plus those characterized as grouped; and finally, the results for Apprehension are a combination of the results which were characterized by the *Os* under all three introspective headings.

It was arbitrarily determined to use for purposes of calculation only those reports which were correct and which had an assurance of 5, 4, or 3. This means that the *O* was sure enough of his report to put even money on it, and this procedure eliminates the doubtful judgments. Therefore, judgments with the assurance of 2 or 1 were eliminated from the calculations, but not from the reports. Two *Os* (YN and MM) worked in entire ignorance of this elimination.

The results for each of the 3 systematic categories were fractionated into 2 groups of 50 judgments for each value of the stimulus in the order taken. The calculations were made in accordance with the phi-gamma hypothesis, and these calculations were simplified by the use of Urban's Tables. Such a calculation yields a statistical threshold as well as a statistical index of precision of the results, for each of our 3 systematic categories, for each of the 4 colors, and for each of the 3 degrees of assurance that were retained.

Observers. Four *Os* were chosen from the staff of the department. They were H. Sherman Oberly (SO), Stanley F. Cooper, (SC), Yale Nathanson (YN), and Miles Murphy (MM), Instructors of Psychology. Two of the *Os*, SO and SC, were well acquainted with the materials and the methods of exposure. YN and MM were trained in giving introspective reports in psychological experiments, but were not trained in this particular experiment.

Results

The tables of the observed relative frequencies of correct judgments for each of the 3 systematic categories and for each of the 3 degrees of subjective assurance—16 tables in all—have been omitted in an effort to conserve space. In every case, the curves approached the ogive type, starting with high relative frequencies for the smaller values of stimulus and descending to small observed relative frequencies for the higher values of stimulus. Very few inversions are noted in the curves and those that do occur are slight. These observed relative frequencies were therefore treated statistically by the phi-gamma hypothesis assisted by the use of Urban's Tables.

Such a treatment yields two sets of values of statistical significance. (1) The statistical threshold or limen (S) which gives that value of stimulus which is correctly reported with a probability of 0.5 or which is as likely to be correctly reported as not. (2) An index of precision (h) which gives an index of the degree of scatter or of the steepness of the curve. These statistical values (S and h) will be found in Tables I to IV.

Each table contains the complete results for a single *O*. Each table is divided into three parts: the upper third containing the results for subjective assurance 5 alone; the middle third for assurance 5 and 4; and the lower third for assurances 5, 4, 3. The results for each color are found in two columns, the first giving the values of the limens (S) and the second the values of the indices of precision (h) respectively. In each third, the first 2 rows contain the results for Attention, the next 2 rows those for Cognition, and the last 2 rows those for Apprehension. The first and second series into which the results have been fractionated are indicated in each case.

TABLE I
Observer SO

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	4.06	1.07	3.90	0.75	4.43	1.15	4.56	1.15
		II	3.64	0.23	3.27	0.21	4.12	1.24	6.02	0.63
	Cognition	I	6.42	0.53	6.31	0.39	6.40	0.60	6.75	0.49
		II	7.39	0.44	7.49	0.45	7.69	0.59	8.12	0.46
	Apprehension	I	7.18	0.56	6.85	0.41	6.73	0.54	7.34	0.46
		II	8.28	0.45	7.99	0.48	8.32	0.54	8.73	0.39
5 + 4	Attention	I	4.06	1.07	3.90	0.75	4.43	1.15	4.56	1.15
		II	3.64	0.23	3.27	0.21	4.12	1.24	6.02	0.63
	Cognition	I	6.59	0.49	6.49	0.34	6.54	0.63	6.97	0.48
		II	7.46	0.44	7.49	0.45	7.77	0.58	8.21	0.46
	Apprehension	I	7.56	0.56	7.07	0.38	6.96	0.57	7.89	0.52
		II	8.38	0.36	8.25	0.45	8.49	0.53	8.97	0.42
5 + 4 + 3	Attention	I	4.06	1.07	4.00	0.91	4.43	1.15	4.56	1.15
		II	3.64	0.23	3.27	0.21	4.12	1.24	6.02	0.63
	Cognition	I	6.84	0.49	6.69	0.36	6.90	0.53	7.33	0.42
		II	7.76	0.38	7.98	0.32	7.91	0.56	8.61	0.39
	Apprehension	I	8.62	0.44	7.98	0.35	8.20	0.44	8.85	0.40
		II	9.02	0.33	9.01	0.45	8.93	0.44	10.31	0.38

The results with regard to the thresholds (S) come out very similar to those of Oberly. The thresholds for Attention are smallest, those for Cognition next, and those for Apprehension largest. This is true for all 4 *O*s and for all degrees of assurance. As one compares the results for the different *O*s there is surprisingly little variation; the average threshold for Attention being approximately 4, that for Cognition, 7 to 8; that for Apprehension of the magnitude, 9 to 10. As one compares the results of the first series with those of the second fractionated series, a certain amount of progressive practice is evident. The degree of increase in the threshold as the result of practice,

however, is much greater for apprehension than for cognition and attention. This would indicate better methods of re-imaging the stimuli, and an improvement in the mediating process.

The examination of the size of the thresholds (S) for SO, Table I, indicates that for all 6 series, Attention, Cognition, and Apprehension, the limen for Red is invariably higher than that for any of the other colors. The position of the other colors is not quite so evident, although Green has certainly the lowest limen (second—1; third—5; fourth—12 of the total eighteen cases). Yellow and Blue are intermediate and not very different. Yellow, however, was in second place more often than Blue (Yellow—11 seconds; Blue—6 seconds). The average position of the colors seems clearly to be Red with the highest limen, Yellow, Blue, and Green with the lowest limen.

TABLE II
Observer SC

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	5.18	1.20	3.94	0.74	4.62	0.57	5.28	0.93
		II	5.73	1.15	5.12	1.72	5.00	1.68	5.42	2.84
	Cognition	I	7.36	0.54	5.24	0.34	6.44	0.30	7.16	0.51
		II	7.85	0.70	7.21	0.56	6.92	0.52	7.89	0.55
	Apprehension	I	7.40	0.54	5.33	0.32	6.49	0.30	7.33	0.50
		II	7.87	0.69	7.04	0.54	6.96	0.52	7.85	0.54
5 + 4	Attention	I	5.18	1.20	3.97	0.74	4.72	0.49	5.28	0.93
		II	5.73	1.15	5.12	1.72	5.00	1.68	5.42	2.83
	Cognition	I	8.10	0.54	5.90	0.32	7.43	0.41	7.85	0.43
		II	8.28	0.71	7.80	0.53	7.58	0.58	8.38	0.53
	Apprehension	I	8.43	0.53	6.40	0.27	8.01	0.27	8.35	0.40
		II	8.56	0.60	8.18	0.51	7.87	0.51	8.61	0.48
5 + 4 + 3	Attention	I	5.18	1.20	4.00	0.73	4.72	0.49	5.28	0.93
		II	5.73	1.15	5.12	1.72	5.00	0.17	5.42	2.83
	Cognition	I	8.02	0.54	7.89	0.42	7.77	0.48	8.05	0.51
		II	8.52	0.66	8.05	0.48	7.80	0.65	8.80	0.61
	Apprehension	I	9.82	0.34	8.25	0.27	8.84	0.38	9.89	0.35
		II	10.89	0.25	9.92	0.28	9.52	0.23	10.26	0.33

The results of SC, Table II, would indicate that Red and Blue are tied for first place with regard to highest limen, each being in the first position 9 times and in the second position 9 times. Average differences between Red and Blue, however, are not very great. These results hold for all 3 categories of Attention, Cognition, and Apprehension, and for all 3 degrees of subjective assurance. Similarly, Green and Yellow fall into another and significantly different group for third and last place. Green has the third highest limen 10 times and the fourth highest limen 8 times; Yellow has

the third highest limen 8 times and the lowest limen 10 times. Again the sizes of these limens are not significantly different from those of the limens for Red and Blue.

In Table III, the results for YN would indicate that Red has clearly the highest limen for all the cases of judgment and for all 3 degrees of subjective assurance—the limen for Red being highest in 14 of the 18 cases and in second place 4 times. A somewhat anomalous condition was found with regard to the relative position of the limens for Yellow and Blue. If Attention and Cognition are alone considered, Yellow is clearly in second place, the score being 2 firsts, 8 seconds, and 2 thirds for Yellow, and 2 seconds with 10 thirds for Blue. The values, however, are not significantly

TABLE III
Observer YN

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	4.32	0.42	4.22	0.58	4.45	0.43	4.46	0.40
		II	5.24	0.68	4.86	0.52	5.30	0.54	5.45	0.67
	Cognition	I	6.14	0.36	6.11	0.33	6.82	0.29	6.41	0.33
		II	7.04	0.30	6.80	0.23	7.11	0.32	7.54	0.26
	Apprehension	I	6.57	0.42	6.32	0.34	7.15	0.39	7.11	0.25
		II	8.16	0.48	8.14	0.44	7.99	0.44	8.81	0.43
5+4	Attention	I	4.26	0.36	4.21	0.44	4.47	0.43	4.50	0.41
		II	5.24	0.68	4.86	0.52	5.30	0.54	5.45	0.67
	Cognition	I	6.95	0.28	6.62	0.28	7.15	0.31	7.02	0.31
		II	7.50	0.25	7.45	0.19	7.49	0.27	8.17	0.21
	Apprehension	I	7.59	0.35	6.95	0.29	8.53	0.38	7.98	0.43
		II	8.58	0.39	8.71	0.41	8.45	0.41	9.37	0.41
5+4+3	Attention	I	4.33	0.36	4.26	0.43	4.47	0.43	4.51	0.40
		II	5.24	0.68	4.86	0.52	5.30	0.54	5.35	0.67
	Cognition	I	7.38	0.25	7.15	0.25	7.40	0.30	7.51	0.25
		II	8.14	0.17	7.75	0.16	7.88	0.21	9.99	0.19
	Apprehension	I	8.26	0.30	8.45	0.35	7.95	0.34	8.48	0.30
		II	9.28	0.35	8.91	0.36	8.82	0.36	9.87	0.36

different. In the case of Apprehension, Blue seems clearly in second place with 2 seconds and 4 thirds for Blue as against 2 firsts and 4 fourths for Yellow, but again, the differences are not significantly different. Green very clearly gives the lowest limen, being lowest 14 times, second twice and third twice.

In the case of MM, Table IV, there is slightly more scatter than will be observed in the results of the other *Os*. However, Red gives, in general, clearly the highest limen, being in first place 11 times, in second place 7 times, and in third place only once. Similarly, Green gives in general, the lowest limen, being lowest 10 times, in first place 4 times, and in third place 4 times. These thresholds for Red and Green on the whole would seem to be significantly different from one another. Blue and Yellow are

clearly intermediate. It is with difficulty that one would determine which had, in general, the highest values. The score stands, Blue first, 3 times; second, 5 times; third, 4 times; and lowest, 6 times. Yellow gives the highest limen once, the second highest, 6 times, third highest, 9 times, and the smallest limen, twice.

TABLE IV

Observer MM

Assurance	Category	Series	Blue		Green		Yellow		Red	
			S	h	S	h	S	h	S	h
5	Attention	I	3.94	0.48	4.27	0.71	4.14	1.14	4.26	0.97
		II	4.61	1.80	4.59	1.86	4.71	2.07	4.90	1.28
	Cognition	I	6.91	0.80	6.58	0.73	6.65	0.71	6.82	0.74
		II	7.54	0.81	7.83	0.68	8.02	0.66	8.93	0.36
	Apprehension	I	6.99	0.79	6.63	0.76	6.85	0.67	6.86	0.77
		II	8.26	0.75	7.96	0.68	7.17	0.21	8.54	0.68
5 + 4	Attention	I	4.05	0.50	4.14	0.60	4.16	1.10	4.26	0.93
		II	4.61	1.80	4.59	1.86	4.71	2.07	4.90	1.28
	Cognition	I	7.25	0.79	6.97	0.67	7.09	0.73	7.23	0.70
		II	9.10	0.95	8.47	0.73	9.45	0.55	9.04	0.43
	Apprehension	I	7.98	0.46	8.76	0.63	7.92	0.59	8.17	0.63
		II	9.03	0.89	8.22	0.57	8.84	0.70	9.58	0.41
5 + 4 + 3	Attention	I	4.01	0.50	4.34	0.60	4.16	1.10	4.28	0.93
		II	4.61	1.80	4.59	1.86	4.71	2.07	4.90	1.28
	Cognition	I	7.24	0.84	7.02	0.63	7.19	0.72	7.33	0.65
		II	8.86	0.96	8.56	0.71	8.88	0.55	9.04	0.43
	Apprehension	I	9.09	0.67	8.67	0.49	8.66	0.59	9.24	0.55
		II	9.96	0.68	10.99	0.25	10.09	0.42	10.60	0.48

Table V clearly indicates the relations discovered with regard to the relative magnitude of these threshold averages.

In every case, Red in general gives a high limen and Green a low one. Yellow and Blue have a tendency to be intermediate and variable. This situation is in general true for all 3 systematic categories and for all 3 degrees of subjective assurance for every *O*.

TABLE V

O	Relative positions of limens			
	Highest	2nd	3rd	Lowest
SO	R	Y	B	G
SC	(R	B)	(G	Y)
YN	R	(Y	B)	G
MM	R	(B	Y)	G

For SC and SO, trained *Os*, there is also observable a characteristic difference between the size of the coefficient of precision (h) for Red and Green. The coefficients for Red are almost invariably larger than those for Green; the individual variations, however, not being exceedingly dissimilar. No such relationship can be discovered for the coefficients for Blue and Yellow. For the untrained observers, YN and MM, however, no such relationship in the size of the coefficient of precision is noticeable.

Discussion

Introspective reports were given by all the *Os* frequently during the experiment. An analysis and consideration of these reports did nothing more than indicate to the *E* that the conditions for the formation of the different judgments remained constant throughout the experiment. There is nothing in the introspective reports which throws any light upon the threshold differences which have been discovered for the different colors. The explanation of these results might depend upon several possible facts.

(1) *Intensity of the colors.* It would seem unlikely that the intensity of the colors would be the correct explanation in the light of the results obtained by Cooper in which he discovered that no constant effect was produced in 3 of his 4 *Os* when he exposed materials of Black, Medium Gray, and Light Gray on a White background.[4] His method and procedure was the same as that indicated in the present study; the only difference being the nature of the materials exposed. If intensity of the present materials were to give the effect indicated, it would be obvious that a still greater effect would have been observable in the uncolored materials which Cooper employed.

(2) *Saturation of the Stimuli.* It will be remembered that in the set of Hering papers, Red and Blue are highly saturated while the Yellow and Green are less highly saturated. Saturation might account for the relationship position of Red giving the highest limen and Green giving the lowest limen, but when one considers the unsaturated nature of the Yellow stimulus and its high position on the liminal scale, it would seem that such an explanation is highly improbable.

(3) *Anklingen times for the different colors.* It will be remembered that Kunkel has discovered that the order of rapidity of the *Anklingen* times gives Red as fastest, Green as slowest, with Blue intermediate.[5] The actual times he gives are, for Red 0.057 sec., Blue 0.091 sec., and Green 0.133 sec. Such a situa-

[4] S. F. Cooper, The effect of brightness in the range of attention experiment, this Journal, 40, 1928, 254-274.
[5] Kunkel, *Arch. f. d. ges. Physiol.*, 9, 1874, 197.

tion would explain very obviously the threshold differences when we consider that we are dealing with materials tachistoscopically exposed for an exposure of approximately 40 sigma. More recently, however, Bills has shown that the order of the *Anklingen* times varies considerably as the result of the methods employed and as a result of the intensity of the stimuli.[6] Bills does find, however, that when one approaches threshold values of stimuli, the order is Red highest, Yellow, Blue, and Green.

We are not prepared to state dogmatically that this is an explanation of our data. It would seem, however, as if the differences in *Anklingen* were a very possible explanation when one considers that the tachistoscopic exposure was shorter than the time necessary for any of these colors to have reached their perceptual maximum.

Summary

(1) The judgments for the range of Attention, Cognition, and Apprehension fall into curves of the phi-gamma type for red, yellow, blue, and green stimuli.

(2) The limens for Attention are smallest, for Cognition intermediate, and for Apprehension the largest, in confirmation of Oberly's results with uncolored stimuli.

(3) The limens for Red are constantly largest for each of the three systematic categories and for the different degrees of subjective assurance. The limens for Green are similarly smallest, with those for Yellow and Blue intermediate.

(4) No such relation is discovered with regard to the magnitude of the values of the coefficients of precision.

(5) Brightness and saturation differences do not seem to account for these liminal differences, and the *Anklingen* times of the colors possibly offer a more satisfactory explanation.

[6] M. A. Bills, The lag of visual sensation in its relation to wave lengths and intensity of of light. *Psychol. Mon.*, 28, 1920, no. 127, 1-101.

THE RELATION OF THE INTELLIGENCE OF PRE-SCHOOL CHILDREN TO THE OCCUPATION OF THEIR FATHERS

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The existence of a rather marked relationship between the results of intelligence tests and the social status of the individuals tested was observed at a very early period in the history of intelligence testing. A report by Decroly and Degand[1] giving the results of the application of the Binet 1908 scale to 45 children of superior social status attending a private school in Paris showed that none were below Binet's standards for their respective chronological ages, only 20% tested 'at age,' while the remaining 80% tested from one to three years 'above age.' In a critical discussion of these findings, Binet[2] calls attention to the marked difference in the social background of these children as compared to the groups used in the establishment of his norms and suggests that this may be an explanation of the apparent discrepancy.

Similar findings have been reported by other workers, both in this country and abroad. The earlier reports are as a rule based upon rather loose descriptive classifications such as "children in a good neighborhood as compared to those in a poor neighborhood," while the more recent studies are likely to adopt some more nearly objective system of classification, such as racial stock, amount of education received, occupation, income, etc. As a simple and easily ascertained basis for classification, the kind of occupation engaged in possesses a number of points of superiority. Since it forms a part of nearly all records, embarrassment or hesitancy is rarely encountered when the information is asked for directly, and in case there is no direct contact with the individual, the data can usually be obtained from other sources. Statistical errors due to incomplete or inaccurate data are thus reduced to a minimum. Moreover, accurate comparative figures for any locality may readily be compiled from the census reports, a matter which is obviously of much importance. A knowledge of the occupation also makes possible a rough estimate of the financial status of the individual, and of the amount of education which he is likely to have received.

During recent years a fairly large number of studies dealing with the relationship between intelligence and occupation have appeared. These studies may be classified under four main heads, as follows:

(1) Studies based upon the judgment of supposedly competent individuals. Of these, the Barr scale for occupational intelligence is fairly representative.[3] One hundred carefully defined occupations were ranked

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[1] O. DeCroly and J. Degand, La mesure de l'intelligence chez des enfants normaux d'après les tests de MM. Binet et Simon, *Arch. de psychol.*, 9, 1910, 81-108.

[2] Alfred Binet, Nouvelles récherches sur la mesure du niveau intellectual chez les enfants d'école, *Année psychol.*, 17, 1911, 145-201.

[3] F. E. Barr, A scale for measuring mental ability in vocations and some of its applications, *Stanford Univ. Publ.*, 1918. Also described in Terman's *Genetic Studies of Genius*, I, 1926, chap. iv.

by thirty independent judges according to the comparative amount of intelligence deemed necessary to achieve an average degree of success in each. These judgments were then converted into equivalent P.E. units by the usual statistical procedure, and the results arranged in the form of a rating scale for occupations.

(2) Studies of the social origin of famous men. Galton's *Hereditary Genius* is one of the earliest of these.[4] Among the more recent studies may be mentioned those by Cattell[5] and Cox.[6]

(3) Studies based upon the actual examination of persons employed in different occupations. Of these, the reports from the Army Intelligence Tests have been most widely quoted.[7] A number of special studies of various groups of civilian employees have also appeared, and there have been several reports based upon the examination of unemployed men. Reports on the relative standing of students in training for various types of occupation might also be included.

(4) Studies in which the performance of school children on standardized tests of intelligence is compared with the occupation of their fathers. Studies which compare the intelligence-test standing of children coming from contrasted social classes less specifically defined (as public versus private schools, good neighborhood versus poor neighborhood, etc.) also belong under this head, since the factors which determine social segregation of the kind mentioned are largely dependent upon paternal occupation.

In spite of minor variations which are probably due in the main to differences in employment standards, to fluctuations in industrial conditions at different periods or in different localities, and similar circumstances affecting the selection of the groups, the essential nature of the results is the same. Whether the comparison is made between persons actually engaged in the occupations considered or their offspring, whether the data are gathered here or abroad, whether individual tests or group tests are used, it is invariably found that the individuals belonging to the professional classes exceed those belonging to the laboring classes in average standing on practically any of the recognized tests of intelligence. Moreover, whenever selective factors can be so controlled as to secure equally representative samplings of the various occupational groups, it is found that fairly well defined hierarchies of mental ability have been established within broad occupational classes of a given type. Thus, expert accountants on the average rank higher than bookkeepers; bookkeepers exceed filing clerks. Skilled mechanics or electricians earn a higher average standing than boiler makers; boiler makers exceed railway track layers. University professors do better than city high school teachers, high school teachers exceed grade teachers. It is, of course, necessary to remember that these statements apply only to the average standing of the groups mentioned, and that these averages

[4] Francis Galton, *Hereditary Genius*, 1869.
[5] J. McK. Cattell, A statistical study of eminent men, *Pop. Sci. Mo.*, 62, 1903, 359-377; Families of American men of science, *ibid.*, 86, 1915, 504-515.
[6] C. M. Cox, The early mental traits of three hundred geniuses, in Terman's *Genetic Studies of Genius*, II, 1926.
[7] *Memoirs of Nat. Acad. Sci.*, 15, 1921, part 3.

must be based upon a sufficient number of cases to insure a reasonable degree of reliability for the findings. There is always a considerable degree of overlapping of scores for the various groups, or, in other words, there are many individual cases which show a marked departure from the average performance of their occupational class. These exceptions do not obscure the general tendency.

At least three fairly distinct types of explanation have been put forth by way of accounting for these findings. The first ascribes the lower standing of the laboring classes to social heredity. It is said that these groups are made up largely of individuals who have lacked educational opportunity and intellectual stimulation. The low standing of their children is explained in like manner, since persons who are themselves lacking in intellectual accomplishments are not likely to provide intellectual surroundings for their children.

The second explanation is in some degree allied to the first, but the emphasis is placed upon the tests as related to the environment, rather than the environment alone. It holds that there are qualitative as well as quantitative differences in mental ability, and that these differences are primarily the outgrowth of differences in life experience. According to this theory, the intelligence tests in common use are unduly weighted in favor of the kind of performance best facilitated by the training and environment ordinarily found among the upper social classes; hence the lower standing of the laboring classes does not imply a lesser degree of ability, but merely that their ability has been developed along other lines not measured by the tests used.

The third explanation, while it recognizes that the factors just mentioned may have some bearing upon the results, nevertheless, places the major emphasis upon differences in native endowment, upon biological rather than social heredity. It takes the point of view that a greater degree of true mental ability is required for success in the learned professions than for success as a day laborer, and considers that industrial selection, rather than differences in opportunity accounts for the tendency toward a segregation of abilities in the various occupational groups. The results of this selective process are carried over into the next generation, since children tend to resemble their parents in mental as well as in physical traits.

Inasmuch as all the studies which have been reported up to the present time are based upon either adults or school children, it has seemed worth while to compare the findings with those obtained through a study of children of pre-school age recently carried out at the University of Minnesota Institute of Child Welfare. A group of 380 children, all of whom were between the

ages of 18 and 54 months, were given the Kuhlman Revision of the Binet tests twice, the two examinations being separated by an average interval of six weeks. A brief application form, which was filled out for all the children, included a question as to the occupation of the child's father. The occupations of adult males between the ages of 21-45 years as listed in the 1920 census report for the city of Minneapolis had previously been classified into six main categories, using the Barr Scale[8] for occupational intelligence and the Taussig Industrial Classification[9] as guides. The percentages falling within each of these categories were then computed and the figures thus obtained have been used as a basis for determining the representative character of groups selected for study at the Institute.

The exact make-up of each of the six occupational categories is stated in the monograph describing the results of the tests.[10] The general nature of the classification may be indicated as follows:

Group I. Professional.
Group II. Semi-professional (accountants, draftsmen, etc.) and managerial.
Group III. Clerical and skilled trades.
Group IV. Semi-skilled trades and minor clerical.
Group V. Slightly skilled.
Group VI. Unskilled labor.

The 380 children included 190 boys and 190 girls. Taking age to the nearest birthday, there were 122 two year olds, 126 three year olds, and 132 four year olds. The distribution as to occupational categories is almost identical for the three age groups. The mean IQ earned by the children of the various occupational categories on each of the two examinations given is shown in the following table:

TABLE I

MEAN IQ OF CHILDREN WHOSE FATHERS BELONG TO DIFFERENT OCCUPATIONAL CATEGORIES

Examination	Group					
	I	II	III	IV	V	VI
First	116.1	111.7	107.7	105.3	104.3	96.0
Second	125.0	119.7	113.4	108.0	107.4	95.8
Number of cases	56	29	129	79	48	39

[8] *Op. cit.*
[9] F. W. Taussig, Industrial classification of occupations, in Terman's *Genetic Studies of Genius*, I, 1926, chap. iv.
[10] F. L. Goodenough, The Kuhlman-Binet tests for children of pre-school age; a critical study and evaluation, *Univ. Minnesota Inst. Child Welfare, Mon. Series*, 2, 1928.

Inspection of Table I reveals two notable characteristics. Not only do both examinations show a steady decrease in the average IQ of the children as we go down the scale of paternal occupations, but this tendency is distinctly more marked in the results of the second examination than in those of the first examination. The fact is one of importance, since it was shown in the course of the main study that the second examination is the more valid of the two measurements. The generally high ratings are due to inaccurate standardization of the tests at the early age levels.

It was felt that it would be interesting to compare these findings with the results reported by other investigators. Three studies were selected for this comparison: the Barr Scale values using the frequencies reported for the city of Minneapolis in the 1920 census, the distribution of Alpha scores for the white draft,[11] and Haggerty and Nash's study of elementary school children in New York State.[12] In the case of the latter two studies, the farming group was excluded, since our data are based entirely upon city children, and it was desired to make the material as nearly comparable as possible. Where medians only were given, these have been treated as the equivalent of means, a fact which undoubtedly introduces a slight though unavoidable error. The standard deviations are based upon the original distributions which include the farmers, since the data were not presented in such a form as to make it possible to correct for the withdrawal of this group. (The distributions of scores for the separate occupations are not shown in either report.) In both instances, the true variability of the group included is presumably somewhat smaller than that of the total group from which this figure was derived.

In order to facilitate comparison, the findings from each study have been converted into standard x/σ scores. In the case of the Army and the Haggerty data, these scores are probably reduced somewhat below the value which would have been found if the farmers had not been included in the distributions from which the standard deviations were derived. The results are shown graphically in Fig. 1. The data of Fig. 1 are given in Table II.

Since both the Barr Scale values and the Army scores have reference to persons actually engaged in the various occupations, although the former is based merely upon a consensus of the opinions of certain individuals and the latter is derived from empirical data, while the Haggerty and the preschool material have to do with the offspring of these persons, it is to be expected that the former would show a much clearer separation of the various occupational classes than the latter, as a result of the law of filial

[11] *Memoirs Nat. Acad. Science*, 15, 1921, part 3, 547-877.
[12] M. E. Haggerty and H. B. Nash, Mental capacity of children and paternal occupation, *J. Educ. Psychol.*, 15, 1924, 559-572.

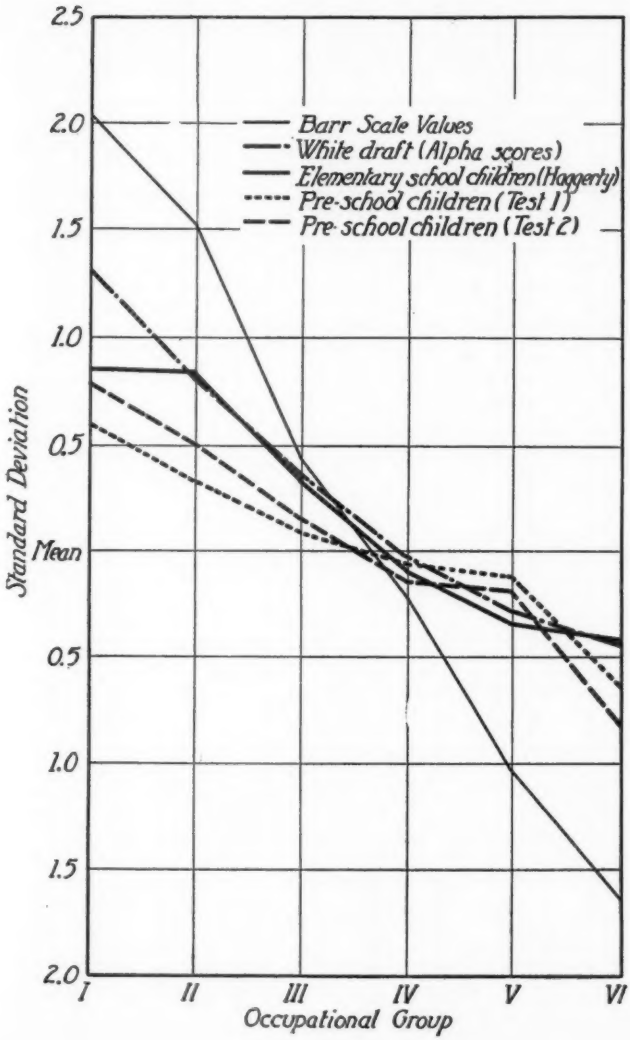
2.5
2.0
1.5
1.0
0.5
Mean
0.5
1.0
1.5
2.0
Standard Deviation
Barr Scale Values
White draft (Alpha scores)
Elementary school children (Haggerty)
Pre-school children (Test 1)
Pre-school children (Test 2)
I
II
III
IV
V
VI
Occupational Group

FIGURE 1

TABLE II

COMPARATIVE STANDING OF THE DIFFERENT OCCUPATIONAL GROUPS ON THE VARIOUS MEASUREMENTS IN TERMS OF RAW SCORES AND THEIR x/σ EQUIVALENTS

	I		II		III		
Measurement	Raw mean	x/σ	Raw mean	x/σ	Raw mean	x/σ	
Barr Scale Values	15.65	+2.04	14.01	+1.53	10.49	+0.44	
Army Alpha Scores (White draft)	113.75	+1.32	94.68	+0.82	76.83	+0.36	
Haggerty elementary school Mean IQ	113.4	+0.86	113.2	+0.85	103.1	+0.32	
Minneapolis pre-school, Test 1 Mean IQ	116.1	+0.60	111.7	+0.33	107.7	+0.09	
Minneapolis pre-school, Test 2 Mean IQ	125.0	+0.80	119.7	+0.50	113.4	+0.15	

	IV		V		VI		Average	
Measurement	Raw mean	x/σ	Raw mean	x/σ	Raw mean	x/σ	Mean dis.	σ dis.
Barr Scale Values	8.38	−0.21	5.71	−1.03	3.74	−1.63	9.03	3.25
Army Alpha Scores (White draft)	62.25	−0.02	52.43	−0.28	46.70	−0.43	63.12	38.36
Haggerty elementary school Mean IQ	95.1	−0.09	90.4	−0.34	89.0	−0.41	96.9	19.2
Minneapolis pre-school, Test 1 Mean IQ	105.3	−0.06	104.3	−0.12	96.0	−0.64	106.3	16.2
Minneapolis pre-school, Test 2 Mean IQ	108.0	−0.14	107.4	−0.18	95.8	−0.82	110.6	18.1

regression. Inspection of the curves will show that the slope of the line for the Barr Scale ratings is much steeper than for any of the others, but that for the army scores is only slightly greater than the ones for the elementary school or the pre-school children. These facts call for further consideration.

It is quite probable that there was a constant tendency on the part of the individuals who made the ratings for the Barr Scale to exaggerate the differences in the intellectual requirements of the various occupations, and that the unusually marked slope of this curve is in part a function of this tendency. On the other hand, it is practically certain that the distribution of Alpha scores for the white draft does not afford an adequate basis for comparison of the occupational extremes, since this distribution does not include the illiterates who took Beta, practically all of whom were found in the lower half of the occupational classification, and whose intellectual level, on the average, was distinctly below that of the group who took Alpha. In the case of the day laborers (Group VI) the median Alpha raw score for those who took Alpha was 46.7, or a C rating; the median Beta weighted score for those who took Beta was 65.8. A rough translation of the Beta score into an equivalent value in terms of Alpha would place it not far from 20.0 points, as compared to 46.7 for those belonging to the same occupational class who took Alpha. Since this group includes 1453 cases, the likelihood of obtaining a difference of this magnitude by chance is negligible. That the selection due to the non-inclusion of the group who took Beta is not equally distributed over the different occupational categories is amply demonstrated by a comparison of the percentages in each occupational class who took Beta as compared to the percentages who took Alpha. In Group I, 5.4% took Beta, 94.6% Alpha; in Group II, 5.3% took Beta, 94.7% took Alpha; in Group III, 7.8% took Beta, 92.2% Alpha; in Group IV, 18.5% took Beta, 81.5% Alpha; in Group V, 24.7% took Beta, 75.3% Alpha; in Group VI, 32.7% took Beta, 67.3% Alpha. It is obvious that in a comparison of the standing of different occupational classes based upon Alpha alone, the lower classes are less clearly differentiated from the general population than would have been the case if the men taking Beta had also been included.[13]

A similar lack of differentiation exists in the Alpha scores at the upper end of the occupational distribution. This is in large measure due to the large proportion of professional men who volunteered for the officers' training camps and were therefore not included in the draft army. It should also be noted that physicians and dentists, who constitute one of the large professional classes, were drawn into a special service. It is true that the average standing of the medical group was somewhat below that of the generality of white officers, a fact which, as Cobb and Yerkes have shown, was probably due to selective factors.[14] Nevertheless, even this group ranks decidedly above the men we have classified as Group I in the draft army. The median score of the medical group was 129, of the dental group 117, of white officers in general 139, of Group I in the draft army 113.75. Since Group I is supposedly made up of men belonging to the

[13] In the table given in the *Memoirs* showing the standing of the men according to occupation, the results for the Beta group are given in terms of weighted scores, those for the Alpha group in raw scores. The reason for this is not clear, since practically all other tables dealing with Beta, as well as those for Alpha are in raw scores, and the table of equivalents used for converting scores earned in one test into their most probable value in terms of another test includes only the raw score values. An attempt to include the Beta group in the distribution according to occupational categories used in this study was discontinued because of inadequate data upon which to base the conversion of scores. The estimate for the laboring group given above is derived by interpolation in the scale of letter grades. It is probably not entirely accurate; but it is unlikely that the error amounts to more than one or two raw score points.

[14] M. V. Cobb and R. M. Yerkes, Intellectual and educational status of the medical profession as represented in the U. S. Army, *Bull. Nat. Research Council*, 1, 1921, part 8.

professional classes for which college training is ordinarily an essential, it is not out of place to compare these figures with the many reports for college students to whom the Alpha tests have been given. Data for more than twenty colleges and universities throughout the country have been summarized by Pintner.[15] The medians for undergraduates average about 135, varying somewhat with the college year (whether freshmen, sophomore, etc.) and with the type of institution. For graduate students in universities of good standing the median is usually above 150. For 97 men in the draft army sample who reported college graduate work it was 146.

It is of course true that the inclusion of the volunteer officers and the Beta group would have somewhat increased the standard deviation of the distribution, but this increase would certainly have been small in proportion to the change in the means at the occupational extremes. In the case of our data, the tendency would be somewhat counterbalanced by the omission of the farming group, as was stated in an earlier paragraph.

Other selective factors affecting the army occupational data should be mentioned. Because of the youth of the drafted men, a large proportion of them had not yet settled down to a life work. Very many of the occupations reported were undoubtedly of a temporary nature and would be considerably modified later on through the operation of industrial selection and personal initiative. This has probably resulted in a tendency toward a flattening of the curve beyond the position which it might fairly be expected to assume later on. Differential selection of men for the 'preferred' occupations in civilian life must also be considered, as well as the possibility that there was some systematic tendency for the men to falsify their statements on occupation in an attempt to secure admission to particular arms of the service. These and other similar points are discussed at some length in Part III of the *Memoirs*.

It appears highly probable, therefore, that the curve based upon the army data is considerably flatter than would be the case if the average age of the men included had been greater, and if the selective agencies mentioned had not been operative. Whether it would have reached the position indicated by the Barr Scale is impossible to say; though from the evidence at hand it seems unlikely that the change would be of quite this magnitude. That the comparative absence of filial regression when the army scores are compared with those derived from the two groups of children is an artifact due to selection seems, however, reasonably certain.

The Haggerty scores show practically no difference between occupational Groups I and II. This also is presumably a result of unequal selection of cases. It was stated in an earlier paragraph that this group included both rural and city children. In order to make the distribution of cases conform more nearly to our group of Minneapolis children, the farmers' children have been omitted in working up the data. While this would exclude the greater number of the rural children, Group I, which is the professional class, would still contain the children of country doctors, school teachers, etc. Because of the strong competition in the cities, there is always a tendency for the less competent members of the professional groups to be forced out into the rural districts, where the cost of living is lower and the competition is less keen. The average rating of the children in the Haggerty Group I is quite probably lower than would be the case had it been made up exclusively of city children from the professional classes. Group II, on the other hand, includes no occupations which are likely to be found in the rural districts, hence the country children are automatically excluded.

The points just mentioned are illustrative of the sort of error which is likely to enter into material of this kind. In

[15] Rudolf Pintner, *Intelligence Testing*, 1923.

spite of minor inconsistencies, however, the five curves in Fig. 1 show a general similarity which is indeed surprising in view of the fact that they are based upon data derived from such widely different population groups, examined by different methods and under quite dissimilar conditions. In every instance, the mean falls between occupational Groups III and IV; a point which coincides with its theoretical position according to the census data for Minneapolis which probably do not differ greatly from those for other northern cities. The upper three groups include 49% of the male population of that city, the lower three include 51%. The mean, therefore, should be located between Groups III and IV as was stated above. It should also be slightly nearer Group IV than Group II, and a glance at Fig. 1 will show that this expectation also is verified. In each of the separate curves the mean for each successive occupational category is lower than that for the one immediately preceding, although in the case of Haggerty Groups I and II the difference is very small. Moreover, in the upper half of the distribution, there is no crossing of the curves based on the different investigations. In the lower half, some slight irregularities occur, chiefly in the case of the army data and that obtained by Haggerty, where Group VI is less clearly differentiated than is the case in the remaining three curves.

In working up the pre-school material, we have presented our findings for the three age groups combined, but treatment by ages separately showed no reliable tendency for the difference between occupational classes to vary with age. Taking the figures at their face value, the clearest separation of the occupational groups was found among the two-year-olds, but this variation from the findings for the children of three and four years was well within the limits of chance. The difference between the means for the two occupational extremes (Groups I and VI) in the case of the first examination given to the pre-school children is almost identical with that shown by Haggerty's data for New York State elementary school children enrolled in grades 3-8. On the second examination, which we have elsewhere shown to be the more reliable of the two measurements,[16] the differences are even greater than those found by Haggerty. The correspondence of both these studies with the army report, making due allowance for filial regression and for the selective factors noted in preceding paragraphs, is also striking.

These data show that intellectual differences between social classes are well established by the ages of two, three, and four years. It is perhaps unnecessary to add the caution that the findings have to do only with the present generation, and with educational and industrial conditions as they are found in this

[16]Goodenough, *op. cit.*

country. Prior to the industrialization of society such differences were presumably much smaller than at present, although the studies by Clarke,[17] Cox,[18] Havelock Ellis,[19] and others show that some degree of segregation along these lines has been operative for a number of generations. The findings of Duff,[20] McDonald,[21] and others demonstrate the existence of a similar relationship between intelligence and occupation in European countries; though the differentiation appears to be slightly less marked than in this country. It is not unreasonable to expect that with increasing specialization in industry and increasing equalization of educational opportunity, differentiation of occupational classes along intellectual lines will become even more marked in the future than has been the case in the past, though non-intellectual factors will always continue to play a considerable part.

The finding of such marked intellectual differences between social classes as early as the age of two years must be regarded as highly significant. The fact that no appreciable change takes place in the position of the various occupational groups from the age of two up to the close of the elementary school period affords strong evidence that the underlying factors, whatever may be their nature or origin, are non-cumulative in their relative effect upon mental development. Furthermore, while one may plausibly advance the hypothesis that the home of low cultural standards does not afford a sufficient opportunity or an adequate stimulus for the acquisition of the more complex and precise language concepts or the fund of general information necessary to achieve a notable degree of success with the tests standardized at the upper age levels, it is less easy to understand the process by which these factors serve as a handicap to the two year old who is judged upon the basis of his response to such simple commands as, "Throw the ball to me;" his ability to name simple objects; or to draw a rough circle with considerable help. Even the least cultured of modern city homes provide, it would seem, adequate opportunity for the acquisition of these early accomplishments of the young child. The fact that children of different social classes show as great differences in their performance of these extremely simple tasks as they afterward manifest in regard to the relatively complex problems of later life, lends support to the theory that under ordinary conditions of modern life, variations in mental growth are more directly dependent upon innate factors than upon differences in post-natal opportunity or stimulation.

[17] E. L. Clarke, American men of letters: their nature and nurture, *Studies in History, Economics, and Public Law*, 1916.

[18] Cox, *op. cit.*

[19] Havelock Ellis, *A Study of British Genius*, 1904.

[20] J. F. Duff and G. H. Thomson, The social and geographical distribution of intelligence in Northumberland, *Brit. J. Psychol.*, 14, 1923, 192-198.

[21] Hector McDonald, The social distribution of intelligence in the Isle of Wight, *Brit. J. Psychol.*, 16, 1925, 123-129.

A COMPARISON OF THE SPANS OF 'ATTENTION' AND MEMORY

By H. Sherman Oberly, University of Pennsylvania

Introduction

In a recent study[1] Guilford and Dallenbach review various methods which have been used to determine the span for immediate memory, and then present a study in which the method of constant stimuli was used to determine the threshold for the immediate memory of 5 *Ss*, and for a group of 100 *Ss*.[2] They conclude that "an exact determination is obtained more readily and more quickly [by this method] than by any of the other methods thus far proposed." Brotemarkle concludes a discussion of memory span problems at the college level with: "the memory span score is not sufficient in itself; it requires a complete analysis of the complexity of the mental processes involved, and assurance of the directness of response."[3] Miller states in his discussion of the diagnostic value of the memory span test that "in the case of higher scores the result has been exaggerated by means of grouping, and the factor of planfulness plays an important part."[4] We observe that Jones used five types of memory span tests in studying superior children in order that she might find the true 'operative' span. She states that "with intelligent subjects, grouping and other devices supplement the true operative span."[5] Easby-Grave also states that "it is possible . . . that the higher spans obtained from older children and adults are the result merely of grouping or organization, and not a true span."[6] Memory span, according to Humpstone,[7] is a test of associability which refers to the "number of discrete perceptions associated in a single act of attention, and the combination of the associated component parts of a single perception."[8]

Problem. The writer became interested in a comparison of the spans of 'attention' and 'memory' while working on the problem of the range for visual attention and the audito-graphic memory span for digits.[9] An inspection of the early results indicated the definite grouping of digits during the reproduction. This grouping had been observed and discussed many times in group experiments, but had never been studied individually. It was considered worthwhile to follow the suggestion of Guilford and Dallenbach to learn if there would be differences, and the nature of these differences, between the limens for the ungrouped memory span or 'attention span,' and the grouped 'memory span' as the test is generally known and used.

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[1] J. P. Guilford and K. M. Dallenbach, The determination of memory span by the method of constant stimuli, this Journal, 36, 1925, 621-628.

[2] The stimulus value for which correct judgments are given in 50% of the cases.

[3] R. A. Brotemarkle, Some memory span problems, *Psychol. Clin.*, 15, 1924, 251.

[4] K. G. Miller, The competency of fifty college students, *Univ. of Penn. Exper. Studies in Psychol. and Ped.*, 8, 1922, 19.

[5] Alice M. Jones, An analytic diagnosis of one hundred twenty superior children, *Psychol. Clin.*, 16, 1925, 44-45.

[6] Charlotte Easby-Grave, Tests and norms at the six year old performance level, *ibid.*, 15, 1924, 279.

[7] H. J. Humpstone, Some aspects of the memory span: a study in associability, *Univ. of Penna. Exper. Studies in Psychol. and Ped.*, 8, 1917, 1-31.

[8] Lightner Witmer, *Analytical Psychology*, 1902, 251.

[9] H. S. Oberly, The range for visual attention, cognition and apprehension, this Journal, 35, 1924, 332-352; Further results in the 'range for visual attention, cognition and apprehension' experiment, *ibid.*, 37, 1926, 132-138.

Method and Procedure

Material. One hundred and thirty series of digits were prepared. There were 10 series for every stimulus value from 2 to 14 digits inclusive. Brotemarkle warns that "the memory span test is of little value except when used by the examiner who has concisely arranged the stimulus with the intention of removing all inherent dangers from the span itself and from the manner of presentation."[10] The different series were arranged so that there would be eliminated, as far as possible, all successive or reverse order of digits as well as number associations; *i.e.* additions, subtractions, familiar dates, etc. An analysis indicates that the number 1 was the first of the series in 3% of the cases; 2, 17%; 3, 8%; 4, 13%; 5, 16%; 6, 23%; 7, 7%; 8, 14%, and 9, 9%.

Procedure. Successive series did not begin with the same digit. *E* read the series in regular order from the first to the last at the rate of approximately 1 per sec., and repeated the procedure until 50 observations had been obtained for every stimulus value. *E* read in a monotone, except for the last digit in the series which was accented as a signal for the end of the series. Inasmuch as the *Ss* could complete a few more than the prepared number of series in a single session, every session began with a different series.

Two methods of presentation, A and B, were used. In Method A the stimulus series of the various lengths were presented in haphazard order—care, however, being taken to see that series of the same length did not follow one another. In Method B the stimulus series were presented in regular or successive order, *i.e.* several series of 5 digits were presented, then several of 6, of 7, etc., in regular order up to and including the series of 12 digits. Method A was used with all our *Ss*; Method B with but one.

There were 5 experimental sessions with all the *Ss* assembled together, of 1 hr. each; two of these sessions were held at 5:00 P. M.; 2 at noon; and 1 at 8:00 A. M. A short rest was permitted the *S* at the end of half an hour's work.

An introspective report was obtained from every *S* after every experimental session. All the *Ss* worked without knowledge of the problem, and, in Method A, without knowledge of the length of the stimulus series.

Subjects. Seven members of the staff of the Department of Psychology acted as *Ss* in this experiment. Messrs. Sanderson (Sa), Humes (H), and Murphy (M), had the greatest amount of training in the introspective work. Messrs. E. M. Twitmyer (T), Altmaier (A), Irwin (Ir), and Gillespie (G) had had no previous experience in this type of work. Mr. Murphy (M) served as *S* in both methods, A and B, of presentation.

Instructions. The following instructions were read to the *Ss* at the beginning of every experimental session. "I am going to say some numbers to you and immediately after saying the last number, when I shall lower my voice, write the series of numbers on the paper which I have placed before you and cover it with the second sheet. Wherever possible, indicate the grouping of digits by means of brackets or spaces. The signal 'ready' will precede the reading of each series of digits."

Results

The observed relative frequencies of the correct reports for the 'attention span' appear in Table I. These results were obtained without any indication of grouping on the part of the *Ss*. The 'memory span' results, which consist of the correct ungrouped as well as grouped reports, appear in Table II.

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[10] *Op. cit.*, 251.

TABLE I

ATTENTION SPAN

Showing for every *S* the percent of correct reports for every value of stimulus

S	Number of digits in stimulus series								
	2	3	4	5	6	7	8	9	10
Sa	100	100	98	16	6	0	0	2	0
H	100	100	88	22	10	4	2	0	0
M	100	100	100	96	98	88	50	10	0
T	100	100	100	20	12	6	2	2	0
A	100	100	100	26	4	2	4	0	0
Ir	100	100	100	34	16	20	8	4	8
G	100	98	98	84	50	16	2	0	0

TABLE II

MEMORY SPAN

Showing for every *S* the percent of correct reports for every value of stimulus

S	Number of digits in stimulus series												
	2	3	4	5	6	7	8	9	10	11	12	13	14
Sa	100	100	98	100	96	78	66	42	16	8	8	0	2
H	100	100	96	70	46	24	14	6	0	0	0	0	0
M	100	100	100	100	100	100	100	90	92	72	58	40	36
T	100	100	100	94	88	70	74	42	22	6	2	2	0
A	100	100	100	100	100	100	94	76	46	24	12	8	10
Ir	100	100	100	100	100	98	96	78	56	52	30	26	18
G	100	100	100	98	92	68	44	40	42	26	8	6	0

In Figs. 1-7 will be found the curves which have been constructed from the results appearing in Tables I and II. The solid line represents the correct judgments for 'attention span' while the broken line indicates the 'memory span' results. The curves for M, T, and G show a slight decrease for an odd number series with a subsequent increase for the next even series. This may be due to the method used in the 'memory span' test.

The values for L (limen) and h (precision) will be found in Table III. The correlation between the L and h values for 'attention span' when placed in rank order is striking, for M and G are the only *S*s to vary. For 4 of the *S*s, the value of h is greater for the 'attention span' than for the 'memory span.' When considering rank order for 'memory span,' the *S* with the lowest limen has the greatest value for precision. The highest limen is accompanied by the next to the lowest precision.

TABLE III

Showing for every *S* the spans of attention and memory

S	Attention Span		Memory Span	
	L	h	L	h
Sa	4.093	0.4039	8.640	0.3413
H	4.693	0.5822	6.032	0.4196
M	7.915	0.5902	13.666	0.2701
T	2.555	0.2433	8.466	0.3474
A	2.816	0.2835	10.161	0.3387
Ir	2.625	0.1592	11.071	0.2754
G	5.954	0.6346	8.701	0.2606

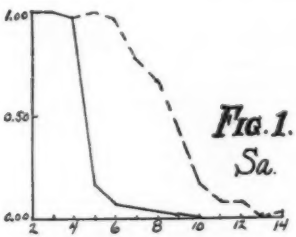


FIG. 1.
Sa.

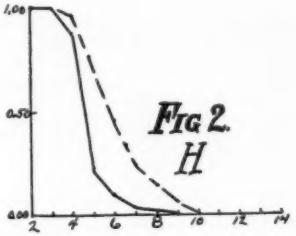
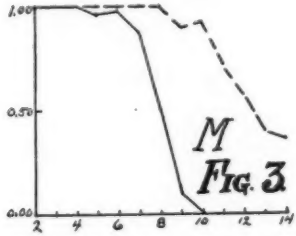


FIG. 2.
H



M
FIG. 3.

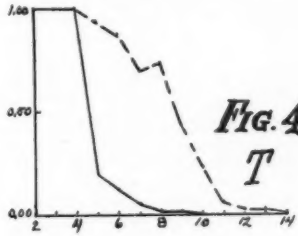


FIG. 4.
T

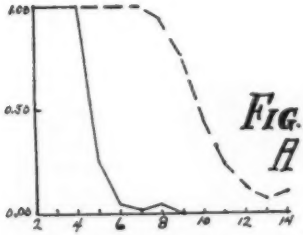


FIG. 5.
A

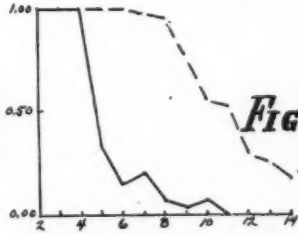


FIG. 6.
Ir

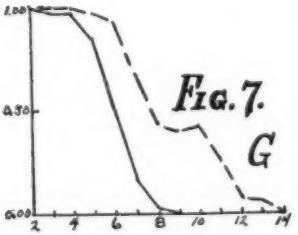


FIG. 7.
G

FIG. 8.
M
(Method B)

'Attention' —— 'Memory' - - - - -

The introspections, when considered individually, would indicate that the *E* read the stimulus series with a rhythm and accent. For example, at the end of the first session, Sa reports, "natural grouping of 3s seemed to be facilitated by the *E*'s reading." M reports, for the same session, "series up to and including 8 digits are reproduced at times without grouping in the reproduction." H states that he "made an effort to group" the digits. *S* A noted "decided grouping in practically every series." For the same session Ir however, states, "I generally group in 4s." G also grouped in 4s, but in a long series he "would often forget the first four digits." At the end of another session, Sa reports, "It seemed to me that the material was read in groups of 3s." A says, "Numbers seemed to be given in a rhythmical manner in 3s, probably due to my own grouping of the numbers." Ir writes, "*E*'s presentation seemed quite without rhythm."

When the introspections are considered collectively, they indicate that the rhythm was not of *E*'s making, that it was subjective, and they thus corroborate the conclusion of Gill and Dallenbach that "grouping is not dependent upon the objective arrangements of the stimuli as much as it is upon the subjective disposition of the *O*."[11] Gill and Dallenbach worked in the visual field, but their conclusion seems to hold also for the auditory field.

Several of the *S*s refer to their lack of preparedness for the series as they are given. Heretofore all had been familiar with the memory span test in which the number of digits is increased with each succeeding or second trial. T remarked on the first day that he "found it hard to get short series because I was prepared for more digits, and when they were not given there was a tendency—not confusion—toward a complete dropping out of consciousness of the numbers." Ir writes, "I sometimes missed 9 because I was expecting 12." G agrees that "if I am set for a long series and the series given is short, the abruptness throws me off and I forget the series."

In order to test whether the method of presenting the stimulus series affected the results a supplementary series of experiments was conducted with one *S* (M) with Method B. In this method, the usual one used in determining the memory span, the *S* could anticipate the number of digits to be given. The following results were obtained. M's 'attention' span with Method A was 7.915, with Method B was 7.566. His 'memory' span with Method A was 13.666, with Method B was 11.381. We do not consider these results final, for the range of the stimulus series in Method B was not as great as in Method A, but whatever advantage there is lies in favor of Method A and not of Method B, as our *S*s had supposed.

In order to obtain further information regarding the two methods, a group experiment was conducted with 100 *S*s. Stimulus series varying from 4-12 digits in length were presented to the group by the two methods and the percent of correct reports were determined for each. These data are given in Table IV. The group limen with Method A is 8.974; with Method B it is 8.929. The difference is slight, but it lies in the direction of Method A, and corroborates the results with M. An inspection of the data obtained from the group shows that 30 *S*s made the same score in the two methods, that 35 *S*s had larger spans with Method A, and that 35 *S*s had larger spans with Method B. We feel therefore that our method of procedure did not materially affect our results.

The degree of assurance was mentioned by 6 of the 7 *S*s. Sa remarks that "assurance was low when my attention was far from maximum." H stated that he had more confidence at the end of any series. M felt "sure of some series of 12 and 14. If I were able to command the maximum attention many more series could be reproduced correctly. My failures are

[11] N. F. Gill and K. M. Dallenbach, A preliminary study of the range of attention, this JOURNAL, 37, 1926, 256.

often due to lack of attention." A noticed that he had "more assurance due to the building up of associations." At the end of the last session, Ir wrote, "I have felt increasingly confident with each hour of work." G noted the low degree of assurance toward the end of one session when he tried out a new method of grouping. On the whole, the assurance increased with the progress of the experiment, and fatigue which was mentioned in the first session was disregarded. H mentioned fatigue but once and only "after a decided effort to *avoid* grouping." All of the *S*s mention the fact that little attention was required for the shorter series of digits.

The grouping, that has been suggested by the investigators referred to above, is brought out rather definitely in an examination of the methods used by the *S*s. Sa mentions an effort to group in 3s (and sometimes 4s) in the first session. In the next session he notes that it was hard to group in 4s. Sa reports further, "without the grouping method I could work easily when the series was short. Any grouping but 3 is an effort, and often it is a distraction rather than an aid. My grouping represents a conscious effort to group the digits in preparation for the reproduction." One might observe that the results for Sa are not indicative of his attention span because of this effort on his part, but we note that he groups in 3s rather than 4s. His threshold for memory span (8.640) would indicate the probable attention span of three groups of three digits each.

TABLE IV

Showing the percent of correct reports for every value of stimulus for the haphazard (A) and successive (B) methods of presentation

Method	Number of digits in stimulus series									L	h
	4	5	6	7	8	9	10	11	12		
A	100	100	98	84	65	44	26	20	10	8.974	0.3392
B	100	100	96	90	69	45	25	12	5	8.929	0.4130

H repeatedly refers to his "conscious effort to group, but in no particular way. I tried grouping in 2s, but would frequently find other groupings apparently arranging themselves as by 4s or 5s." "This morning without apparent effort, groups of 3 began to appear in consciousness . . . . throughout the session the 3 grouping prevailed."

M, who has the highest limen for both the attention and memory spans, states that "it is comparatively easy to reproduce 5 digits without grouping" after he mentions that "in order to make certain in case a series should be long, one naturally groups." He further states that "some series are obtained by memorizing the first group of 4 digits." "It is easier to make associations on the basis of mathematical processes; *e.g.* the series 8-5-1-7 comes to me as one fifth of 85 is 17." "7 and 8 at the time of reproduction exist in consciousness as discrete units. Higher series are grouped." One should compare the observations and results of Sa and M in order to see clearly the different spans which we are presenting.

T had had no experience in this method, and his reports are meager. He "tried to group in 2s and 3s but lost out on the long series. On the short series I started with 4s, but when it came to recall the natural group was 3." He alternates between 3s and 4s throughout the experiment.

A writes, "I tried to group in 4s and I found myself referring to the series as dates, *i.e.* 1685, 1862, etc." Later in the experiment we find another effort to use dates in order to reproduce the series. In a series of 12 digits—3-7-1-5-2-8-1-6-4-9-5-7—he states that "37 is more than twice 15 and 28 is less than twice 16." Here we have a scheme which is similar to that suggested by M.

Ir starts out by grouping in 4s. "Grouping by 4s seemed to me to be much more efficient than by 3s." Toward the end of the experiment he writes: "I have now built up a number of association forms, into one of which I can press almost every series, thereby making it more probable that I can reproduce it. I attempt to memorize the first 4 digits in some way and keep the last ones by straight grouping." Later on he states that "I have become quite proficient in memorizing the first 4 digits of each series."

G reports: "I usually tried to group the first 4 numbers in groups of 2 and to use association for the others. Once I tried to interpret the digits as notes on the musical scale and tried to hum the tune of the digits: *i.e.* 1 = C, 2 = D, 3 = E, etc. The association worked for 3 digits, but I could not transpose fast enough to follow the presentation. At another time he had new associations. "The numbers of my apartment are 81, 82, 83 and 84. When one of these numbers appeared together, I associate them with the room." He varied the method from two groups of 2 each to three groups of 2. He observed a definite effort to group into 2s, 3s or 4s.

We find that this grouping is a definite mental process in this experiment. An inspection of the limens for 'attention span' for each of the *Ss* in Table I will show the results obtained by the various methods described by the *Ss*. M is the only one who reports that he finds it rather easy to get the digits up to 8. Although this may seem a rather large number, we must keep in mind the fact that his threshold for 'memory span' is a value of 2.6 digits above the next highest span of the *Ss*. All of our observations indicate that M has the highest range of any of the *Ss* selected in this investigation.

Humpstone states that "the subjects must have enough image-ability to get perceptions of the stimuli," and that "the images must be retained for the brief period of reproduction."[12] The elapse of time is just sufficient for writing the series, so that there is slight opportunity for a repetition of the series. Various types of imagery are reported by the *Ss*. Sa states that it is chiefly auditovocomotor and that visual imagery was of little aid to him. H reports, "at times I tried to visualize the digits as written on the sheet of paper used as a shield, but this was finally discarded as impractical." M writes, "I tried using visual imagery but without success. All reproduction is based on auditory or vocomotor perseveration." T says, "Some series are given back by use of visual imagery for the first part of the series and auditory imagery for the latter part. The last two digits are obtained frequently by means of an auditory hangover." A first mentions imagery in the third hour; he says, "I seemed to visualize the series to a greater extent this time." At the end of the last session he writes, "I relied on my auditory imagery more than visual. This seems easier." Ir states that "generally when taking memory span tests I go very much by visual imagery, but this seemed lacking almost completely and I had to use auditory." Later on he mentions that his visual imagery was much clearer. G reports auditory imagery which is described above.

Former investigators have generally agreed that the memory span score that is obtained is not a true measure of the number of separate and discrete units which may be grasped and reproduced in a given moment of attention. In a previous study of the range for visual attention the writer concluded that "attention may be characterized by an immediate process in which all the sensory elements have an equal and high degree of clearness."[13] This was the result of simultaneous presentation of 'separate units.' The present investigation follows the method of successive pre-

[12] *Op. cit.*, 15.

[13] Oberly, *op. cit.*, 353.

sentation of separate units.[14] The limens for the 6 *Ss* in the range for visual 'attention' varied in that study from approximately 3 to 5 dots, and we find that the attention span in this study ranges from approximately 2.5 to 6 digits for 6 of our 7 *Ss*. We may expect that the highest *S* for memory span, will have a higher range than any of the other *Ss*, when working with visual stimuli, if the preliminary series can be considered as indicative of future progress.

Conclusions

(1) Individual methods of grouping are used in memory span tests for the longer series.

(2) 'Attention span' is a term which satisfactorily describes the immediate reproduction of separate discrete units.

(3) A brief inspection indicates little difference in test results whether digits are presented in haphazard (Method A) or serial order (Method B).

(4) Visual imagery is of little aid in reproducing longer series of digits.

(5) 'Attention span,' for most individuals, is short enough to cause grouping of units after the number five has been passed.

(6) The 'attention span' limen ranges from 2.5 to 8, while the 'memory span' limen ranges from 6 to 13.5.

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[14]Cf. Gill and Dallenbach, *op. cit.*, 247.

THE CREATIVE YEARS

By Helen Nelson[1]

Goethe at seventy-nine once said of Napoleon, "But the real focus of his lustre belongs to his youth. That one of obscure origin should so have distinguished himself as to become in his seven-and-twentieth year the idol of a nation of thirty millions,—yes, yes, my good friend, one must be young to do great things! And Napoleon is not the only one!"[2]

"One must be young to do great things!" It was a sentence impulsively spoken, and not without its profound truth. Keats dying at twenty-six, Schubert before he was thirty-two, Alexander, with the world conquered, at thirty-three, Christ at thirty-three also, crucified yet with his incalculable influence upon the centuries, disclose the extraordinary power which may be released to youth. Yet we do not commonly think of early deaths otherwise than as frustrations. "Losing the spring out of the year"—Pericles' touching figure for the loss to the State of its youth in war—is after all losing its fruit. And Goethe as a matter of fact, in subsequent conversation and elsewhere, modified materially that one swift sentence quoted.

Yet the unmodified thought has currency, and Professor Woodworth of Columbia University in his *Psychology* has given it matter-of-fact expression. Speaking, in his chapter on *Imagination*, of the favorableness of youth to invention, "Seldom," Professor Woodworth says, "does a very old person get outside the limits of his previous habits. Few great inventions, artistic or practical, have emanated from really old persons, and comparatively few even from the middle-aged. The period from twenty years up to forty seems to be the most favorable to inventiveness."

That the years between twenty and forty are productive ones for genius there can be no doubt. But that the manifestations of genius are mainly confined to this score of years, genius itself disproves. A list of the great poets of Europe, for instance, might conceivably include the following nine great names: Homer, Aeschylus, Virgil, Dante, Chaucer, Shakespeare, Milton, Molière, Goethe. Though Longinus has an interesting passage in which he declares that "Homer grown old" wrote the *Odyssey*, "Homer great as ever, like the great setting sun, but without his fervent heat," we may best pass over Homer. The life of Aeschylus offers less for conjecture. Aeschylus winning his first victory at forty, and his thirteenth

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[1]The author of this paper, said of it, in two letters received some time after it had been accepted for publication, that "It grew out of reading of my own and was a steady two-months' labor of research and writing. At the same time the paper was by no means my own entirely, for my intelligence at every point was working with some other intelligence which seemed reënforcing mine. I mean that there was a constant transference of thought during the research for this paper and the writing of it. But whose intelligence it was directing mine I have no idea. The same condition has been true in the writing of one other paper of an entirely different nature, the editing of a book [actually published], as well as in very many experiences of ordinary life. This whole manner of living, which is fundamentally evil, for it means a secret power by person or persons unknown over body and mind and is made possible through hypnotism, animal magnetism, electricity or something very akin to these, first became distinct to my consciousness when by such person or persons I was made insane. I have reason to believe now, however, that these influences were active long before my recognition of them. Latterly it has been suggested to me by these same invisible, unwanted partners that my experience may be far from unique." The author has requested that the paper should appear under a fictitious name. The foregoing statement adds to the interest of the paper, which was accepted on its intrinsic merits. No editorial corrections have been necessary either in its admirable style or in the data which it presents. —M. F. W.

[2]*Conversations with Eckermann*, March 11, 1828.

and last and greatest victory at sixty-seven, but in his epitaph recalling only the proud serving of his youth in war, may cast just a passing light upon the sentence of Goethe's already quoted. Forty was a landmark for Virgil also, for then he began the *Aeneid*. Dying at fifty-one, with the poem as we have it now, he wished the manuscript burned, and wrote this into his will, because his work must lack the careful revision he had planned of three more years. Just when Dante began and finished his *Divine Comedy* is not known, but when he was about forty-nine he was working on the last cantos of its central portion, the *Purgatory*. Professor Skeat places the central period of the *Canterbury Tales* in Chaucer's late forties. Careful scholarship assigns to Shakespeare's forties *Othello, Lear, Macbeth, Anthony and Cleopatra, Coriolanus, Cymbeline, The Tempest, The Winter's Tale*. Milton began his *Paradise Lost* at fifty. Molière, dying at fifty-one, did not come into his genius until he was forty. "Nothing," suggests Mr. Saintsbury, "is more instructive than to compare *Les Précieuses Ridicules*, which is almost his first play, with *Les Femmes Savantes*, which is almost his last (the former produced at thirty-seven years, the latter at fifty). They are so closely connected in subject that the later play has sometimes been called an expanded recast of the earlier. But the improvement in treatment is immense. Amusing as *Les Précieuses Ridicules* is, it is not much more than farce of the very best sort. *Les Femmes Savantes* is comedy of the highest kind, the result of exact observation of life informed by intimate knowledge of character, and clothed with the most accomplished phrase."[3] As for Goethe, *Hermann und Dorothea* was written at forty-seven and forty-eight years, the first part of *Faust* was finished when he was fifty-seven, its second part when he was eighty-two. So that in the case of these great names at least, invention of the highest order, far from being in decay at forty, seems at very prime or just ready to begin.

Suppose we list some of the great tales of the world. Such a list would undoubtedly include *Don Quixote*, the first part of which was sent to press when the author was fifty-seven, the second part—"no book more signally contradicting the maxim that 'No second part was ever good' "[4]—when he was sixty-eight. If we include La Fontaine's *Fables*, it is the second collection, published when its author was fifty-seven, not the collection of a decade earlier, which called out Madame de Sévigné's "Divine!" The first part of the *Pilgrim's Progress* appeared when Bunyan was forty-eight, it being now generally supposed to have been written in the year immediately preceding; while the second part, taking up the tale of Christian's wife and their children, did not appear till Bunyan was fifty-six. *Robinson Crusoe* had a youthful author of sixty or almost sixty. *Gulliver's Travels*, said to have been well on its way toward completion when Swift was fifty-three, was not published until he was fifty-nine, though there seems to have been a period of private circulation; the first hint of *Gulliver* came to Swift when he was forty-seven. Sir Walter Scott did not enter upon the period of his novel-writing until he was forty-three, *Ivanhoe* appearing at forty-nine, while, as it happened, it was almost the latest of his novels, *The Fair Maid of Perth*, published when he was fifty-seven, which Goethe praised to Eckermann with such lively and intimate enthusiasm.[5] *Les Miserables* met an eager world when Victor Hugo was sixty years old; when *Quatre-Vingt-Treize*, so highly praised by Swinburne, was published its author was two-and-seventy.

Or suppose we were asked to name the great orators of the world. Probably the names of Demosthenes, Cicero, Burke would be first on our lips. But two of Demosthenes' greatest speeches, one of them the third

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[3] G. Saintsbury, Molière, *Chambers' Encyc.*, 1896.

[4] J. F. Kelly, Cervantes, *Encyc. Brit.*, 11th ed.

[5] *Conversations with Eckermann*, Oct. 3, 1828.

Philippic, were delivered when he was forty-two or three years old; while eleven years later, his whole public life under attack, he gained "an overwhelming victory for himself and for the honor of Athens" (they are Professor Jebb's words) "in the most finished, the most splendid and the most pathetic work of ancient eloquence—the immortal oration *On the Crown.*"[6] As for Cicero, he was forty-three in the year of his four famous speeches against Catiline, and over sixty when he delivered against Antony those *Philippics* which cost him his own life. While Burke made, at forty-six, his great plea for the conciliation of America, and opened the famous trial of Warren Hastings with a masterpiece of English eloquence at fifty-nine. Nor can we pass over the Gettysburg address, spoken when Abraham Lincoln was fifty-four.

Of the great composers of music, many met early death, Schubert before he was thirty-two, Mozart at thirty-five, Mendelssohn at thirty-eight, Chopin at forty, Schumann after several years of broken health at forty-six. But the *Messiah*, by which we are wont to remember Händel, was written when he was fifty-six; and we have forgotten his earlier forty-odd operas. Bach's *Passion according to St. Matthew*, to name but a single celebrated work, was written at forty-four. At sixty-three and onward Haydn was writing "the last and finest" of his chamber music, and at sixty-seven the *Creation*, surviving to-day alone of his oratorios. "At the beginning of 1827," writes Mr. Tovey (in 1827, the year of his death, Beethoven was fifty-seven years old), "Beethoven had projects for a tenth symphony, music to Goethe's *Faust*, and, under the stimulus of his newly acquired collection of Händel's works, any amount of choral music, compared to which all his previous compositions would have seemed but a prelude."[7] The four great symphonies of Brahms were of the period lying between forty-four and fifty-three years. Wagner was forty-six when he finished *Tristan und Isolde; Das Rheingold* and *Die Walküre* were produced when he was fifty-six and seven; *Siegfried* and *Götterdämmerung* followed when he was sixty-three, and *Parsifal* when he was sixty-nine years of age. As for Verdi, he was fifty-eight years old when *Aïda* was produced; but *Otello* produced at seventy-four and *Falstaff* at eighty years of age are, in the opinion of his biographer, Streatfield, writing in the *Encyclopedia Britannica*, "an immense advance on anything Verdi had previously written," while his settings, at eighty-five, of the *Ave Maria*, *Laudi alla Virgine*, the *Stabat Mater* and the *Te Deum*, to recur again to Streatfield, "in imaginative power and musical beauty yield to none of Verdi's works."

Of the great artists, Giorgione died at thirty-four, Raphael at thirty-seven, Correggio at forty, Van Dyck at forty-two; and Holbein at about forty-six, leaving, as work of his forties, a number of portraits "very perffight," "verye lyvelye." But many of the great artists have lived to sixty years or over, productive long after the brief score of years allowed them by Professor Woodworth is past. Giotto, solemnly appointed three years before his death master of the works of the cathedral of Santa Maria del Fiore and official architect of the city walls of Florence and of the towns within her territory, was able to plan two architectural works of importance and superintend their beginning before his death at seventy (or possibly it was sixty-one) years. Leonardo da Vinci's *Last Supper*, "for all Christendom the typical representation of the scene,"[8] was the work of the four years from forty-two to forty-six; the famous *Mona Lisa* the work of another four ending at fifty-four years. It was in his fifties also that his great cartoon of *The Battle of the Standard* was worked out and exhibited—the goad and inspiration of contemporary artists, but,

[6] G. C. Jebb, Demosthenes, *Encyc. Brit.*, 11th ed.

[7] D. F. Tovey, Beethoven, *Encyc. Brit.*, 11th ed.

[8] Colvin, Leonardo de Vinci, *Encyc. Brit.*, 11th ed.

aside from studies, surviving today only in a tiny partial sketch of Raphael's preserved at Oxford, and in some copies of cherished copies, one of these by Rubens. It was from fifty-nine to sixty-six that Michelangelo painted *The Last Judgment*, "probably the most famous single picture in the world,"[9] and from sixty-seven until his death at eighty-nine that he was chief architect of St. Peter's Church at Rome, with other vast architectural schemes also claiming his attention in the later years. The date of Titian's birth has been questioned, and the very last of his work is said to show failing power; but at least he had not lost the cunning of hand and eye in the year 1562, when at seventy-two or three, as some say, or at eighty-five as he himself professed, he painted the fine portrait of himself cited by William Michael Rossetti as among the highest examples of his work in this kind. Tintoretto began at fifty-nine the memorable series of paintings for the Scuola di San Rocco concluded seventeen years later only with his death; painted at sixty "four extraordinary masterpieces" in the Sala dell' Anticollegio; and at seventy began the vast *Paradise* (seventy-four feet by thirty) which, before the commission was awarded he prayed God he might paint, if so he might himself come at last into Paradise. It was the crowning work of his life. Rubens dying at sixty-three, Velasquez at sixty-one, passed with undimmed powers. As for Frans Hals, some of his most striking work was produced in the long bitter poverty of those later years when, it is thought, he could not afford the more expensive warmer pigments but must perforce use black and white; and in the two canvases of *The Regents and Regentesses of the Oudemannenhuis* (at Haarlem), he created "masterpieces of color" in all but monochrome at over eighty. Rembrandt at fifty painted some of his greatest pictures, including the famous *Portrait of Jan Six*, and at about fifty-seven produced, in the *Jewish Bride* of the Ryks Museum and the *Family Group* of Brunswick, "the last and perhaps the most brilliant works of his life."[10] "The most splendid period of Murillo's career" was that of his commission for the Almhouse of St. Jorge, for which, at fifty-seven, he received for his eight greatest pictures there about £800. Of English artists, Gainsborough did not come into his style until he was over forty; Sir Joshua Reynolds finished at sixty-one *Mrs. Siddons as the Tragic Muse*, "undoubtedly his greatest portrait;"[11] William-Blake's *Illustrations to the Book of Job*, holding first place among his artistic work, were published when he was sixty-nine. As for Turner, his "central and best period," according to the Scottish painter, Sir George Reid,[12] began at fifty-four when he exhibited *Ulysses Deriding Polyphemus; The Fighting Téméraire* and *The Slave Ship* were exhibited at sixty-four and five; while Ruskin in the first volume of *Modern Painters* cites a number of pictures falling between the years sixty-five and sixty-nine as Turner's most surpassingly perfect work. It may be of interest also to refer in passing to the fourth volume of *Modern Painters* and to the plates there reproduced of Turner's earliest *Nottingham*, published at twenty when he was "still working in a very childish way," and his latest *Nottingham* published when he was fifty-eight. As for Corot, and Corot must close a list already overlong, not until about forty-four did he develop "that style which marks the works of his maturity," and his medals at the Salon were won at thirty-seven, at fifty-two, at fifty-nine and at seventy-one years.

In the field of theoretic and practical science also, the later years, with the earlier, play their part. The groundwork of the Copernican theory, laid by its author between the years thirty-three and thirty-nine, tested with means scanty enough through the following years, was not finally

[9] *Idem.*, Michelangelo, *op. cit.*
[10] J. F. White and P. G. Konody, Rembrandt, *Encyc. Brit.*, 11th ed.
[11] Artide on Reynolds, *Chambers' Encyc.*
[12] Article on Turner, *Encyc. Brit.*, 11th ed.

embodied in the *De Revolutionibus Orbium Coelestium* until Copernicus was fifty-seven years old. Nor was the "Little Commentary" or abstract of his thesis written until that year. The latter part of Galileo's life was full of invention. He was forty-five when some rumor of a telescope newly invented by an obscure Dutch optician set him pondering one night on the principles of refraction. Six months later he had an instrument, made with his own hands—he was able to make hundreds of them subsequently for the requests poured in from every part of Europe—by means of which, and his own genius at discernment and exposition, were laid the foundations of modern descriptive astronomy. The first of Galileo's telescopic discoveries took form in the *Sidereus Nuncius* when he was forty-six; his last, that of the moon's diurnal and monthly librations, was made only a few months before he became hopelessly blind, at seventy-three. Even then, remembering the oscillations of a bronze lamp which at nineteen he had seen in the cathedral of Pisa, he thought out the application of the pendulum to clockwork, and shortly before his death (at seventy-eight) discovered the mathematical demonstration of the "not very obvious principle" which earlier it had been necessary for him to take for granted, that the velocities acquired in descending, from the same height, planes of every inclination are equal. While at seventy-two perhaps his most valuable and lasting treatise was completed, the result of his early experiments and mature deliberations on the principles of mechanics—the dialogue of *The New Sciences*. Kepler at thirty-eight gave to the world his "First and Second Laws," at forty-eight his "Third Law," at fifty-six the *Rudolphine Tables*, which for a century retained their authority and in principle have never been superseded; while his *Mysterium* published at twenty-five proved an error. It is supposed that Sir Isaac Newton was in his twenty-fourth year when, according to Voltaire who had the famous story from Newton's niece, the apple fell, directing the young mathematician's attention to the subject of gravity. But his calculations resulting only in serious discrepancies, he "laid aside at that time any further thoughts of this matter." Apparently it was not until he was forty that he recurred to the subject, a more exact figure for the diameter of the earth having in the meantime been formulated, and not until he was forty-two that he wrote *De Motu*, which contained in twenty-four octavo pages the germ of the *Principia*. The *Principia* itself was written in the two years, memorable for science, when Newton was forty-three and four. Darwin, beginning at twenty-eight the patient collecting of fact and testing of hypothesis, did not put his notes in order and publish the *Origin of the Species* until he was forty-nine and fifty years old; and of the series of studies which followed, *The Formation of Vegetable Mould through the Action of Worms* (the completion of a much earlier paper) appeared, at seventy-two, in the year before his death. Jenner, after more than a score of years of study and waiting, made at forty-seven his first inoculation for smallpox. Pasteur was fifty-eight when he set himself to study hydrophobia and sixty-three when he began for this disease the practice of inoculation. Virchow, "father of modern pathology," in the last thirty years of his life (he died in 1902, eighty-one years old) was conspicuously instrumental in effecting for Berlin that great reform in sanitation which left the city, in the beginning one of the unhealthiest in the world, one of the healthiest.

Among the very many investigators who have contributed to our knowledge of electricity or to its practical application, the following names are among those, perhaps, best known: Gilbert, who in 1600, at sixty, in his *De magnete, magneticisque corporibus, et de magno magnete tellure*, laid "the foundations of modern electric and magnetic science;" Benjamin Franklin, whose memorable experiments filled an interval of his forties and fifties between the business from which he had retired and the im-

portant diplomatic services of his later life; Cavendish, whose unpublished electrical researches in 1771 to 1781, when he was forty to fifty years old, were thought of sufficient value, a century later, to be edited by James Clerk Maxwell and published; Volta, whose Voltaic pile, "a fresh implement of great power" to the chemist, was devised when he was about fifty-four; Oersted, whose discovery at forty-two of the effects of a current of electricity on the magnetic needle was "epoch-making in the degree to which it stimulated other research;" Ampère, whose first notable memoir, the direct outcome of Oersted's discovery, was communicated to the Academy at forty-five; Faraday, whose brilliant and unprecedented work, the direct stimulation of untold scientific and technical activity, filled a score of years between forty and sixty; Morse, who at forty-six exhibited his telegraph to a few friends in the University of the City of New York, and at fifty-one was making experiments with telegraphy by a submarine cable; Lord Kelvin, among whose multitudinous achievements were the siphon recorder, designed at forty-three for receiving messages over the Atlantic cable, and (as d'Arsonval's galvanometer) now in common use in electrical laboratories, and a mariner's compass adding greatly to the safety of navigation and generally adopted by the navies of the world, patented in 1878 when he was fifty-two; Röntgen, who at fifty discovered the X-rays, now so indispensable to surgery and, it seems, to art; Edison, Marconi, whose wizardry somehow we are not accustomed to regard as entirely of a past tense.

A handful of inventors in various fields must close a protracted tale. John Harrison at sixty-eight, after a life-time of patient labor, claimed and finally received a reward of £20,000 offered by the British Government forty-eight years previously for a ship's chronometer which would determine longitude with an error not greater than thirty minutes. Rewards of £10,000 and £15,000 had at the same time been offered for chronometers determining the longitude within sixty and forty minutes. The perfecting of Harrison's delicate instrument, outstripping all hope for it determined longitude within eighteen minutes, was the work of twenty-six years after his first chronometer was submitted and found distinctly promising, at forty-two. Although James Watt's first patent for his steam engine was taken out when he was thirty-three, nearly four years as a matter of fact after the inventions covered by it had been made, other important patents followed; the fourth of which, covering the well-known "parallel motion," taken out when he was forty-eight, Watt considered "one of the most ingenious simple pieces of mechanism" he had contrived. Nor was this ingenuity lacking in the very last work of his life—the invention of two machines for reproducing sculpture, one for smaller likeneses, one for likenesses of the same size—with which, not many months before his death, he was making copies of busts and presenting them to his friends as the work "of a young artist just entering on his eighty-third year." George Stephenson's first locomotive, "My Lord," the work of his thirties, was not a particularly efficient machine; his engine used on the first passenger and freight railway to be opened (in 1825 when he was forty-four years old) had a speed of nearly sixteen miles an hour; his "Rocket," which, when he was forty-eight years old, bore off the prize of £500 offered by the Liverpool and Manchester Railway then nearing completion, had a speed of thirty miles an hour. "Now," exclaimed one of the directors, "has George Stephenson at last delivered himself." Photography began in 1814 with Niepce, who at forty-nine produced the first permanent sun-pictures; and with his subsequent associate, Daguerre, who a quarter of a century later, at fifty, announced the discovery of the process, the first of the sort of any practical value, which is associated with his name. Roebling at forty-five began the erection at Niagara Falls of the long-span wire suspension bridge which eminent engineers of the time regarded as

foredoomed to failure, completed at sixty-one the still more remarkable suspension bridge at Cincinnati over the Ohio, and died at sixty-three from accidental injury before finishing his great Brooklyn Bridge over the East River. Nobel patented dynamite in 1867 at thirty-four, blasting gelatine, a more powerful explosive, at forty-three, and produced ballistite, one of the earliest nitroglycerin smokeless powders, at fifty-six.

The inventors are only a handful, chosen from out the baffling silences of our encyclopedists. But they suggest that inventors are not of different race from genius in other fields (nor from humanity for that matter, to whom continued effort means naturally increasing excellence in accomplishment). They suggest, too, that we scan a little more closely the dates commonly associated with inventions in this field. The date of the final perfecting of John Harrison's chronometer, for instance, represents a date preceded by creative activity; yet a date of a type rarely receiving emphasis in accounts of mechanical invention. We crown invention with its earliest date, forgetting that improvement may follow improvement through a lifetime, perhaps, as the relatively crude first contrivance grows into the perfected machine. Such a method of dating is no index to the creative years. Nor is it strictly comparable with the method of dating in theoretic science and the arts, where datings mark normally the finished masterpiece, after the last stroke of the laboring file. Certainly this is a matter to be searched into, in any further study of the creative years. In the meantime we may suggest it as possibly accounting for the barrier of forty which we found raised.

But surely we may suggest that this barrier should be taken down! And to the writer at least, it seems an obstruction best not raised at any subsequent milepost. For here differences in the ageing of individuals must play their subtle part, and health and the possession of sight and hearing contribute, at times a little mockingly; for though a blind Sir Joshua Reynolds cannot paint, yet a deaf Beethoven can compose music, and delicate health, as in the case of Darwin, need not interfere with late creative activity. Then, too, personal relationships, with their stimulus, have also their fortuity, Michelangelo's sonnets, for instance, for the most part falling within a period of five years just before and after he was seventy, when his lonely life was sweetened with the friendship of the accomplished Vittoria Colonna. Debt, as for Sir Walter Scott, or poverty, as for Frans Hals, may conceivably constitute another determinant. The personal ideal must be allowed its definite shaping power also; as with Racine, retiring and silent for over a dozen years before producing when about fifty, at the solicitation of Madame de Maintenon for her girls to act at Saint-Cyr, *Esther* and *Athalie*, two of his finest plays. Destiny, too, has its force. It caught Milton at the Civil War, only his early poems written, into seventeen long years of polemic and diplomatic English and Latin prose; it caught Benjamin Franklin into diplomacy, and George Stephenson into the problems of railway administration. Again, at times one may suspect the possibility of genius solving its problem before exhausting its powers; and observe the new lease of life accorded those who, like Michelangelo, can turn from sculpture and painting to poetry and architecture, or like William Morris, from poetry to the beautifying of living through fine artisanship, as in the latest achievement of his life, the Kelmscott *Chaucer*. At least it is not age alone which halts genius; or turns it from invention in its narrower sense into other, and sometimes wider, thoroughfares.

Nor for Professor Woodworth is age the fundamental factor, but age because bound by habit. "Seldom," says Professor Woodworth in the passage already quoted, "does a very old person get outside the limits of his previous habits." But is it, after all, habit which is the deterrent of genius? Genius, so wary, so impatient of the habitual, so instinctively seizing upon the disparate elements of new and right combinations avoiding

habit, can genius be said to become enchained by its habits of youth? Its habits of youth are habits toward freedom. Its whole approach to life is individual, unique; and its successive approaches to life retain this vital quality. Even the style of genius, its "habitual" expression, grows with its growth, painters and musicians having their successive "manners," Shakespeare writing at different periods a distinguishable music into his lines. The very definition of genius seems to involve something of the protean kind. When genius grows sluggish, as it did early in Wordsworth's life,[13] or breaks down as it did late in Turner's and Titian's, may we not suspect the presence of some subtle disintegrative force among the bodily forces which prevents the operation of habit, or which is even actually dispersing it, rather than the fettering quality of a distinguished creative past? Indeed, it was just as Turner's sight began to fail at seventy, with the health of body and mind, that his work failed also. As for Titian, who continued to paint up to his death at ninety-nine (or was it at eighty-six or seven?), it scarcely seems unnatural to attribute Titian's failing power to some breaking of the delicate bodily habits which for so long had served him. Even with Wordsworth it seems more reasonable to suppose some volatile element in the creative habits of his best decade, lacking which his later work fell to ground, than that he was bound hard and fast by these best ten years. For he continued to compose verse: why did it not continue to be poetry? The later life of Emerson affords still another example. A book had been promised, but it was found that Emerson could not put it together from his own earlier notes. Scarcely could one say that, because he could read those notes but could not fuse them, he was bound by his previous habits of reading. Emerson had behind him the habits of a lifetime of creative composition; it was because these habits were broken with broken health that he was helpless. No, *genius is stopped, in youth or age, when the habits of its personality break down.* While they function, the past but gives enablement, the very munitions of war, for present inspiration. This is only a natural law of growth.

On a December evening after Goethe was eighty years old, he read a scene from the second part of *Faust* to Eckermann, and after reading said, "I have had it in my mind for fifty years. But perhaps it is well that I have not written it down till now, when my knowledge of the world is so much clearer. For I am as one who had in youth many small pieces of copper and silver money, who constantly exchanging them for better, beholds now the property of his youth in pieces of pure gold."[14] Emerson turns us an obverse side of the picture of which Goethe too had knowledge. "We must prize our own youth," he tells us in that volume which he himself was unable to marshal. "Later, we want heat to execute our plans. The good-will, the knowledge, the whole armory of means are all present, but a certain heat that once used not to fail, refuses its office; and all is vain until this capricious fuel is supplied."[15] The pure gold of maturity, the heat of youth—these together make the living masterpiece. Happily, with most men of genius, youth does not die with the years of youth. Continually we have found genius equalling or surpassing its youthful work; while instances are not rare of youth's proving only a latent or apprenticeship period. "Other people are young but once," Goethe explained to Eckermann, "but if the soul is of a powerful kind as is the case with all men of natural genius, then its animating penetration of the body tends to confer the privilege of a perpetual youth. So that we constantly perceive in men of

[13] "Wordsworth composed verses during a space of some sixty years; and it is no exaggeration to say that within one single decade of those years, between 1798 and 1808, almost all his really first-rate work was produced." Matthew Arnold: *Wordsworth*, in his *Essays in Criticism*, 2nd series.

[14] *Conversations with Eckermann*, Dec. 6, 1829.

[15] Emerson, Inspiration, in *Letters and Social Aims*.

superior endowments, even in their old age, fresh periods of singular productiveness; they seem to grow young again for a time, feel a new youth."[15] We can begin to understand therefore, why masterpieces of the first rank are so often produced in the middle and later years. Youth holds over; and maturity with its wealth of winnowed and delicately winnowing habits has been won. To natural endowment is added the gathered wisdom of an intensely critical experience, so that the favorableness of youth to great invention seems more than matched by the favorableness of age. Certainly forty lays no staying, withering hand upon the creative powers. Certainly forty and the years after are years most precious for earth's inventive, as secondarily for her uninventive, children.

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[15] *Conversations with Eckermann*, March 11, 1828. The quotation is somewhat condensed.

APPARATUS

The Victor Stroboscopic Disk for Visual Experiments

The psychologic and world-classic trade-mark "His Master's Voice" shows our canine friend not only listening but looking. The Victor Talking Machine Company in their 60-*Cycle Stroboscope Disk* have provided us with an important item not to listen to but to look at. This paper disk, 5 in. in diam., is for testing the speed of phonograph turntables. On its face 92 radiating black spokes are printed. The black spokes are 2° wide and 1 7/8 in. long. The white interspaces are approximately 1° 45′ wide and there is a clear white space in the middle of the disk 1 1/4 in. in diam. The edges of the spokes are sharp and straight and the entire design seems to be very accurately made. The paper disk is placed on top of a regular record the outer portion of which is then played. The illumination must be from an electric lamp on 60-cycle current and then the speed regulation of the turntable is varied until the dark radial lines appear and are stationary, when the rate will be 78 revolutions per minute. (A 60-cycle current gives 120 light pulses per second or 7200 per minute; 78 revolutions of 92 spokes gives 7176 passing a given point per minute.) The bars appear of about the same intensity as if one looks at the stationary disk through two thicknesses of 16-pound white English bond paper. The method is of course simple and quite exact. Whereas, the ordinary stroboscopic disk[1] provides for making determinations over a range of several frequencies, the present disk provides for just the one rate. Several years ago Lorenz[2] pointed out the applicability of such disks for measuring alternating-current frequency. Quoting him further he said, "stroboscopic methods of measuring frequency and speed of rotation possess certain advantages, and doubtless the reason they have not been more generally used has lain in the lack of a convenient and reliable source of light. Since that time the alternating 60-cycle current for household uses has become so relatively steady that the stroboscopic method can now be used in every household and the Victor Company is to be congratulated for having worked out this simple scheme.

This 5-in. disk considered as a *stationary visual stimulus* is the most *live* object of the kind I have ever seen. If one fixates the center as steadily as he can, he observes phantom objects rippling and revolving in a most extraordinary manner. It resists being converted into a first rate negative after-image except from very brief fixation. There are faint transitory concentric color bands seen near the central portion. The thin black wedges near the center show strikingly the effect known as Bergmann's Experiment.[3] The wedge-shaped spokes are very black and straight at the periphery of the disk, but as they approach the center they seem to become prominently beaded, at times appearing twisted like a thin tapering screw.

If the surface of the disk is viewed almost edgewise, color mixing by irradiation can be observed on both sides while straight across the middle the black spokes show with exceeding prominence. According to the fixation position these prominent spokes show in their true wedge-shaped character as straight lines or as curved lines.

Viewed from a distance of about 15 ft., the disk serves admirably as a test chart for measuring astigmatism and probably does it better than other charts devised for the purpose by reason of the fact that the sector units are so small.

[1]E. W. Scripture, Elementary course in psychological measurements, *Studies from the Yale Psychol. Lab.*, 4, 1896, 136, Fig. 19; also C. E. Seashore, A voice torroscope, *Univ. of Iowa Studies in Psychol.*, 3, 1902, 18–28.

[2]C. F. Lorens, A stroboscopic frequency-meter. *Electrical Review and Western Electrician*, May 17, 1913.

[3]Helmholtz, *Physiological Optics*, trans. 1924, II, 34-35; and Edmund C. Sanford, *A Course in Experimental Psychology*, 1895, pt. i, 107 (experiment 118).

Used as a *moving visual stimulus*, the disk exhibits some remarkable qualities. A very slow rate of rotation provides perfect color mixing (a middle gray) due to the fact that the sectors are only 2° in width. Moved fairly rapidly up and down over a distance of 2 in. while the center is being fixated, brings out a striking illusion of motion. When the disk moves upward, the sectors directly at the bottom seem to expand and grow farther apart. While those at the top execute the opposite motion of squeezing in and becoming nearer together. When the disk is moved downward sharply, the opposite of this accordion-like effect takes place; likewise the effect may be observed if the disk is moved along any other linear axis.

If the disk is moved as on an eccentric without being actually revolved on it's own center, grayish phantoms appear near the central portion and exhibit prominent polar projections which appear to revolve; sometimes in one direction, sometimes in another, almost as a reversible windmill illusion.[4] The disk may well serve as an alternate to the black and white spiral disk[5] for showing colors in connection with flicker, Purkinje's 'shadow figure,' and the apparent arrest of movement due to eye-movements. The Victor disk gives all of these phenomena more prominently. Probably this is due to the tapering shape of each unit and to the radiating character of the whole arrangement. The disk, inexpensive and readily available as it is, should become one of the most useful objects for visual demonstration. The phenomena are so prominent and so bizarre that people as a rule object to looking at it for any but very brief intervals.

Stanford University WALTER MILES

A CHILD'S STYLUS MAZE

The ordinary slot maze[6] is ill-adapted to certain work with children because the *Ss* can 'peek' at the maze or are disturbed if blindfolded. Carr[7] designs a metal maze with invisible stops which avoids these difficulties. The writer has found, however, that Carr's maze is more easily and cheaply constructed out of vulcanized fiber-board, and wishes therefore to make the recommendation. In addition, the use of fiber makes it a simple matter to employ a brass stylus to give electric contacts with the stops, so as to work a signal at the goal, or even to record all errors except reversals in the correct path.

The maze is constructed by inserting smaller pieces of fiber between the base and the blocks which form the surface of the maze, and attaching each block to the base by two bolts.[8] This construction leaves inverted T-shaped grooves which may be obstructed at appropriate points by pairs of bolts screwed through the base into the lateral (hidden) portions of the groove.[9]

Yale University JOHN M. MCGINNIS

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[4]Since preparing this note the article by Carmichael and Schlosberg has appeared (this JOURNAL, 40, 1928, 128.)

[5]Helmholtz, *ibid.*, 256 (Fig. 53).

[6]The 'Foster' maze is an example. cf. W. S. Foster, *Experiments in Psychology*, 1923, 17-18.

[7]A. H. Carr, The influence of visual guidance in maze learning, *J. Exper. Psychol.*, 4, 1921, 398 ff. Another form of this maze was demonstrated by Miss Frances Bodkin in connection with a report given at the December meeting of the American Psychological Association at Philadelphia in 1927.

[8]The author suggests the use of 3/16 in. stove bolts with a No. 24 twist drill, which makes it unnecessary to tap the holes.

[9]From the Psychology Laboratory, University of Maine.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

The Meaning of Psychology. By C. K. OGDEN. New York and London, Harper Brothers, 1926. Pp. 326.

Dr. Ogden's book applies a consistent principle to the field of psychology, especially to problems now under discussion. The style is informal without loss of technical exactness. An admirable clarity, restraint and common sense marks the whole work. Critical comment is frequent but always subordinated to the main issue. The author shows in high degree a power of holding to the essentials of any matter with which he deals.

The nature of his objective leads at the outset to a discussion of the mind-body relation. Seven theories are stated and the defects of each briefly noted. The criticism of these hypotheses seems grounded, in some instances, upon their metaphysical implications rather than any psychological insufficiency. Holders of the Double Aspect Theory, for example did not substitute a speculative thing-in-itself for the psychological objective, though the Double Language view which the author adopts phrases the essential situation better. It is one and the same life which psychology and neurology study.

The author has no quarrel with Behaviorism for making the most of its principle. Its insistence upon objective methods and controlled observation has been altogether healthy. It has compelled Psychology to set its house in order. The study of response patterns and their conditioning has been undertaken by opponents as well as by representatives. Its endeavor to dispense with 'mind' and 'experience' is laudable in view of the unverifiable speculations of the past. This exclusion, "a bias as extraordinary as that which it was designed to avoid," is the fundamental defect of a strict Behaviorism. Psychology depends upon two methods, neurological and introspective, each of which supplements the other. Behaviorism will soon forget its theatrical gesture in disowning experience, just as it will correct its own over-emphasis of movement and recognize the significance of the total psychological fact which 'reaction' includes. What the Behaviorist overlooks is that it is the postulation of satisfaction and dissatisfaction, impulses, aims and valuations which defines the limits of his field and gives him his *point d'appui* as psychologist.

The program of the book is a synthesis of neural conceptions which shall serve as a schematism, objectively for the reaction field and subjectively for attitudes in all their complications. Beginning with the simplest path taken by a nervous discharge its first problem is found in the inhibitions which result from a conflict of impulses. The treatment is based on Lapicque's theory of chronaxies, which starts from a comparison of the times required to produce a response in different nerves and muscles, and of the influence upon these time relations of variations in stimulus-intensity, fatigue, drugs, modifications of glandular secretion, etc. A muscle and its motor nerve are isochronous, but each muscle and each nerve has its own chronaxy. When conduction takes place within any plexus, the chronaxies involved are either identical or have simple numerical relations. If the chronaxy of any element is accelerated or retarded without a corresponding modification in the rest of the field it is thrown out of tune with the system and conduction does not occur. This conception he applies to the selective and organizing functions of the nervous system.

Of the innumerable impulses coming in from the sense organs few reach the final common path of motor neurones, but very many play a part by barring out or modifying concurrent impulses. Lower level receptive centers react to specific patterns as a lock to its key. Associated with these, on the next higher level, are sensory coördinating centers so related that the patterns getting through one center will depend on what patterns have been picked out by others. The key-patterns obviously can neither be simple nor unvarying, and the schematism must from the outset be conceived as very complex. The interdependence of the coördinating centers points to a still higher control mechanism which is found in the associative areas of the brain. The set of the whole system thus determines the history of any impulse coming in, which may illustrate the influence of expectation on perception or that of a general conception on the particular instances which are brought under it.

Pavlov's work on conditioned reflexes affords occasion for the fullest development of this concept. The author finds McDougall's drainage theory and that of lowered resistance alike inadequate. Differences in potential and reduction at the synapses do not account for certain inhibition phenomena which these conditioned reflexes present. What has not been sufficiently considered is the end-state of the whole process. The impulses do not come to an indifferent brain but one tuned to them by hunger, and the total conditioning of the reflex includes its satisfaction. It is the nature of this final state which maintains or disturbs the synchronizing of the neurone chronaxies in which the establishment of the conditioned reflex consists.

In the remainder of this part of the work the conception of need and end-state is applied to such problems as recognition, interest and foresight. The term 'interest' marks our characteristic selection of patterns—which again records the plan of persistent accords in our nerve centers—an activity which depends finally upon the satisfaction which their picking out affords us as integral individuals, their consonance with conscious or unconscious purposes.

Now purposiveness is a psychological characteristic due to retention. What we call foresight is the influence upon any action of the effects of what happened on previous occasions under similar situations. Its essential features appear in the conditioned reflex and involve no non-physical concept, such as a psychic urge or libido.

The general scheme of all these activities is to be found in a few primary needs and the great array of derivative needs associated with them in the case of man. An interest may thus be defined as an activity set going and maintained by a need. The process demands, of course, a nervous organization capable of translating the need into activity, including both excitability in the centers and delicacy and plasticity in their tuning.

This schematism is clearly applicable to the phenomena of selection and exclusion in the field of perception, and serves to re-state Koffka's contribution. The *Gestalt* is an end-state system and new *Gestalt*-patterns will appear wherever fresh end-states or novel systems arise in the satisfaction of needs. The application of the principle to learning is obvious and leads the author through a review of Köhler's experiments with apes to mental growth in man.

Development begins with sensory experience but its 'perception bridges' between need and satisfaction are soon supplemented by images which arise as substitutes for the missing situations and constitute the matrix out of which memory develops. The conflict between fantasy and reality, together with the processes concerned in their integration, is finely drawn.

From this the author turns to look inward upon experience and outward upon the world. The mind is a system of mobile equilibria among tensions, stresses and impulses grounded in needs and moving toward their satis-

faction. This character determines the range of awareness as well as the focus of attention, and affective quality is affiliated with the flow or hindrance of movement toward these end-states. The nature of our outward look has already been indicated, for example in the discussion of interests, and the author turns to the psychology of scientific fictions and the significance of the human equation in all accounts of the external world.

The flow and conditioning of thought reflects in detail this pragmatic principle. It is a process of experimentation with the mental representatives of things, including emotions and moods as well as images. The spread and direction of association are determined by interest and attitude. Remembering and forgetting have a similar dependence, as Freud pointed out. The hierarchy of concepts corresponds to the system of coordination levels for handling impulses. Judgments and beliefs are idea-structures formed in the service of our interests.

Affectively toned experience and emotional attitudes as well as trains of ideas are to be construed as preparations for action and probably constitute the matrix out of which the latter have arisen. The emotional disturbance, however, does not mark situations which allow of a prompt and free response but those which involve a conflict between rival tendencies. This interplay, with its subtle and far-reaching effects, has been the chief object of psychoanalysis in its study of repressions, complexes and defence systems.

This brings the author to his final problem—how the mind goes wrong. In general it may be described as due to the mutual obstruction of interests, resulting in blocking, displacement and loss of integrity in the system as a whole. Many needs which cannot be expressed because activities grounded in other needs are in possession of the appropriate paths find satisfaction either in rational compromise and development or in fantasy and regressive organization. It is the nature of the compromise which distinguishes the normal action from the abnormal. Artificial fixations appear, mechanisms arise which are out of adjustment with more general requirements, the movement toward satisfying end-states is thwarted, the activity of the higher coördination centers is weakened and regression to an infantile condition takes place with widespread disorder as its result.

The book is a valuable contribution to current discussion and the reader will find few topics upon which some interesting light is not thrown.

New York University ROBERT MACDOUGALL

The Journal of General Psychology. Edited by E. B. TITCHENER and CARL MURCHISON. Worcester, Mass., Clark Univ. Press. Quarterly. Vol. I, no. 1, Jan., 1928. Pp. 192.

The appearance of a new psychological journal is always an event of importance to psychologists. *The Journal of General Psychology* is to be a quarterly devoted to "experimental, theoretical, clinical, and historical psychology." This first number includes eleven articles, of which six are experimental, four theoretical, and one on the border-line between the two. There are in it no clinical or historical articles, and the Board is not chosen so as to emphasize the clinical aspect.

The first number is extremely interesting because of the high average quality, the distinction of many of the authors, and the variety of the subject-matter. Six of the Editorial Board—Bartlett, Dunlap, Hollingworth, Miles, Spearman, and Wheeler—contribute articles. Of these articles the only one that can be called 'experimental' is Bartlett's, a fact that verifies a growing suspicion that the correlation between publication of experimental work and distinction of author is negative. There are three articles from Cornell and its periphery, and two from Stanford, but the other six are distributed in a most catholic manner among Clark, London, Cambridge, Hopkins, Columbia, and Kansas.

The journal was 'co-founded' and this number 'co-edited' by Titchener and Murchison. There is no doubt of Titchener's great interest in the project and the general form of the journal bears the stamp of his views. It is most regretable that at the moment of its birth the new undertaking should be handicapped by his death.

The editors are supported by a Board of twenty-four psychologists: sixteen from the United States, three from Great Britain, and one each from Canada, Australia, Germany, Austria, and Italy. Presumably the journal will be primarily American and secondarily Anglo-American.

There is a department of Notes, under Titchener's editorship, and a department of Book Reviews, under Nafe's editorship. In this number the four notes and the seven reviews all come from Cornell or Clark, except one note from Wittenberg. Two of the notes are by Titchener, his last publications.

The journal does the world the service of printing a photographic reproduction of the Brauner portrait of Titchener, an excellent reproduction. It promises later an article on Titchener by "a friend and fellow-psychologist."

One hesitates to make any criticism at all of a new editorial venture, embarrassed before it went to press by the death of its senior editor; but friendly suggestions are always in order. In general the proof-reading is as excellent as the contents. The present reviewer is bothered by the absence of authors' names from the page-headings; the reader presumably knows that there is a *Journal of General Psychology* or he would not be reading it, and he does not need to be reminded of the fact on every even-numbered page; what he wants in a scientific journal is to be able to find quickly an article by a given author without having to use the index or to translate the title into its author. Institutions and their addresses are put in prominent italics at the end of each article: it seems odd to separate the author so far from his address or to give the address such prominence. In general, though not always, quotation marks have been made to follow the laws of logic rather than those of typographical convention; that is to say, punctuation is put within the marks when it is quoted and without when it is not. If this change is a reform, well and good—even though it bother the eye.

Questions of consistency are the bane of the editor's life. When should the author have his way and when not? There is no general answer, and yet consistency seems desirable when it does not interfere with meaning. The reviewer believes that the editors of the new journal should seek conformity in the following matters. Some rule for quotation marks should be adopted and rigidly followed. Common words should be spelled alike: Husband says "thru" and "tho," but no one else adopts the radical orthography. If the rule is to put the laboratory origin of the article in a foot-note and the author's institution and address at the end, then a different usage should not have been adopted for Bartlett. Titchener invented *O* and *S* as translations for the German "*Vpn.*" (and its variants), but the Cornell and Clark articles use "*O*" and the others write out "subject." Most of the references are complete, but Spearman omits page references. Two articles cite the literature by a "Bibliography" at the end and a parenthetical reference to it by number; all the other articles, when they cite literature at all, use foot-notes. Nevertheless consistency is but a minor matter for editors to worry over. *The Journal of General Psychology* will stand or fall by its content of psychology, whatever its typographical uniformity.

E. G. B.

Mental Tests in Clinical Practice. By F. L. WELLS. Yonkers-on-Hudson, World Book Co., 1927. Pp. x, 315.

This book is one of those rare and delightful demonstrations of complete mastery of the art of scientific authorship. The author moves with masterly precision through a welter of literature carefully classifying arranging and evaluating so that those who follow in his foot-steps may profit most by his experience and genius. He is quick to appreciate and slow to condemn, is gracious even when he disagrees and is enviably clear in expression. The result is so thorough a summary of the literature of mental testing that hereafter no psychologists will care to work long without having Wells within easy reach.

The reviewer doubts if one can easily over-emphasize the serviceableness of this book. The reference lists are extensive and are arranged in perfect bibliographic form. There is a general reference list at the end of the volume of 159 titles and in addition there are supplementary lists with most of the chapters which bring the total number of titles well above four hundred. At the end of nearly every chapter there is a presentation of typical cases which taken together number fifty-five. These alone would be a valuable contribution. They show quite clearly how much, and at times how little, the psychometric scores contribute to a complete understanding of particular case problems. The practical tester and the clinical psychologist will also find the tables include, especially those in the appendix, matter of much every-day working value.

Of course there is a chapter on general methods and another on the Stanford Scale, but there is also a full discussion of the Kuhlmann Scale. There is also a summary of the controversy over the possible merits of the point-scale method. The problems and findings of group testing appear in one chapter. The so-called performance tests occupy another. The author's own method for memory examination is reproduced in one chapter, the literature on the free association experiment is digested in another, and then there are two final chapters on personality tests and the vocational uses of tests supplemented by a very pratical presentation of office methods for the clinical psychologist which is inclusive to the most minute detail. In all of this the author is not much inclined to express his own opinion. He keeps himself well in the background, so well in fact that one sees the current trend of discussion and practice in each chapter almost forgetting that he is seeing it all through the critically evaluating eyes of the author. Occasionally, however, the author's own preferences or opinions appear. He defines a mental test as being a psychological experiment which has been standardized. Concerning the much discussed constancy of the I.Q. he admits that there are occasional changes of rating but thinks that in the hands of competent examiners such variations may be safely discounted. In the controversy over the average age of adult intelligence the author thinks that 14-year performance most nearly expresses that of the average adult. On the significance of scatter the author is cautious. He says that he has found scatter so common in normal children that it cannot be safely stressed as a pathological sign; but when adults scatter over five or more year levels he thinks it proper to raise the question of organic or toxic involvement. He is careful to add, however, that a regular performance cannot be interpreted as excluding such involvement. But interesting and important as all this is the merit of the book lies not so much in the author's own opinions and preferences as in the survey which he gives of the general trend in the psychological world of mental tests and the almost innumerable aids which he supplies to the wise use of psychometric methods.

University of Oregon EDMUND S. CONKLIN

L'Intelligence. By BOURDON. Paris, Libraire Félix Alcan, 1926. Pp. 338.

The volume is devoted to a study of higher processes which the author distinguishes from sensory phenomena because of the fact that the latter are the primordial phenomena from which the higher processes are derived, in the sense that they presuppose sensory data. Due to the fact of experimental accessibility, the sensory processes are comparatively well understood. He notes the difficulties involved in the method of introspection, especially the difficulty of separating what has been observed from that which has been inferred. Nevertheless, he does not propose to abandon introspection, but promises to make a serious effort to separate the phenomenal from the inferred. Despite the fact that our knowledge of the higher processes is incomplete, what we do know about them is today far beyond the knowledge contained in old treatises of psychology.

He notes the rudimentary state of our knowledge regarding the cerebral accompaniment of thought. He pleads this fact as an excuse for his relative neglect of neural conditions. He charges those who discuss intelligence in terms of neural processes with merely accumulating hypotheses—to the reviewer's mind, a well-grounded objection to many writers. Surely we cannot dispel our ignorance concerning the mystery of thought by data ('synapses,' etc.) regarding which we know even less. The author observes that to explain thought processes in neural terms generally amounts to substituting new designations for familiar terms, without otherwise 'helping' the situation.

The mental contents he examines (as shown by the succeeding chapters) are immediate images (immediate memory); representations, types of imagery in which he includes Ribot's affective type. There is a chapter on *Sentiments Intellectuels* in which he considers imageless thought, the *Bewusstseinslagen* of the Germans, and the problem of *meaning* as handled by English and American psychologists. There are chapters on perception, association, judgment in general, and a special chapter on judgments of resemblance, chapters on illusion and hallucination—in fact it is, with the exception of chapters on sensory phenomena, a complete textbook. However, emphasis is placed on higher processes and he does bring in impartially data from many foreign sources. His data has the virtue of being genuinely psychological data—he neither tries to substitute neural mechanics for mind, nor to substitute chapters from a logic text for psychology of the thought processes. Both of these errors are rampant within our shores.

Wells College C. O. WEBER

Two Souls in One Body? By H. H. GODDARD. New York, Dodd, Mead and Co., 1927. Pp. ix, 242.

Readers of this journal have doubtless been aware for a long time that Dr. Goddard was working on a genuine case of mutiple personality and will therefore welcome this publication of his study. The story is the now familiar one of a girl in delicate health subjected to excessive tension and emotional distress culminating in later adolescent years in a thoroughgoing breakdown. Two personalities were clearly manifest, one that of a child and the other of a girl in later adolescence. Under mental tests one gave a mental age of four years and the other of sixteen years. Another state appeared a few times which some less cautious investigator might have exploited as a third personality. The interpretation offered is in terms of Janet's theory of a weak synthesis dissociated by emotional shock. The dichotomy of character traits and memories is well established.

There is an ample and interesting presentation of the behavior of the two personalities and of the process of changing from one into the other. The history prior to the break is exceptionally well summarized.

The case is in many respects much more simple than the famous Beauchamp case and is more concisely presented. It is illuminated by some instructive photographs of the patient in different states and also of her hand-writing. The book will therefore be of great service, especially as a type case for student reading. It was a happy thought also to include in the appendix a reprinting of the long famous Mary Reynolds case, thus making that oft quoted report much more accessible.

But the design of the author was not exclusively that of producing a clean-cut report of an instructive case for scientific uses. His aim was avowedly that of using the case as the basis also for an appeal to the general public for a better understanding of the problems of child health and of the public responsibility for its care. He also hoped that he might deal popular superstitions an effective blow. These two supplementary aims have quite naturally influenced both the style of the book and the selection of the material. Here and there the author drops into an hortatory vein which is sometimes almost impassioned. There are also included in the body of the text letters from ignorant cranks, aroused doubtless by the newspaper publicity which the case has received, with many more in the appendix, as well as a curious discussion of the problem of "two souls" which is reflected in the title selected for the book. While no one would question the desirability of awakening the general public, if possible, to a clearer consciousness of its responsibility in such cases, many scientific readers may question the wisdom of attempting to present so valuable a case as this in a fashion to appeal at once both to the tyro and to the technician.

University of Oregon EDMUND S. CONKLIN

Die Persönlichkeit des dreijährigen Kindes. By ELSA KÖHLER. Leipzig, S. Hirzel, 1926. Pp. ix, 240.

The author designates Karl Bühler's developmental theory as the one which is central to the present study. In Bühler's *Geistigen Entwickelung des Kindes* (Jena, 1924) occurs an analysis of the one-year-old child, and Charlotte Bühler has extended his basic principles to the study of the period of puberty. The present study is offered as a connecting link between the two above studies. The author maintains that the third year presents us with remarkable strides in development. The 'third year' in this book is defined as the age lying between 2 years and 6 months and 3 years and 6 months. As regards method, this study is an acceptable product of observation, experimentation, and theoretical interpretation. She ignores mass data, and her empirical findings are drawn almost entirely from her intensive study of one case (*Annschen H.*), a little girl of normal antecedents and character.

The research portions of the book are three in number. The first portion (covering the period from 2.5 until 2.7) deals with the manner in which this child evolves and represents her 'world-view' (*Weltbildes*). However, this involves very concrete observations regarding the child's sense perception in general, color discrimination, space and time perception, and her conception of number. This includes also the child's conceptual world —her notions of law and causality. Also her language mechanisms, vocabulary, grammar, imagery. Part II is concerned with finding a synthetic view of the convergence of external impressions upon inner development. This study covers the period 2.8 to 2.9 inclusively. The author then discontinued her study until A was 3.0 years old. The third study (3.0 until

3.6) is devoted to research regarding A's recollections of the earlier periods, her new attitudes, etc. The chief method employed throughout is that of carefully questioning her subject.

Out of the vast detail of personal data which the study displays regarding this child, we may indicate the following as the most general findings. The first period is dominantly phenomenological. A's senses are not fully developed, she lives in a 'present' world since the past and future are extremely dim notions to her, she has a vague notion of time, the mystery of number is wholly unsolved. Feeling and willing are barely distinguishable. Development is very rapid, however, and in the second period she has already developed a sense of personal identity, which appears *pari passau* with the clear separation she makes between herself and objects, her separation of present from past time, her recognition of relationship to her parents, etc. Her willing and feeling center around this new self-awareness. In the third period, the self is well established, and interests (volitional and affective) turn towards the external world. The impulse *to know* this world—to put it in a scheme—becomes dominant. She holds a criterion of value; the valuable is that which can be understood, which can be grasped as an element in a schema.

Wells College C. O. WEBER

Psychotherapie. Ein Lehrbuch für Studierende und Ärzte. By MAX ISSERLIN. Berlin, J. Springer, 1926. Pp. 205.

This psychotherapy represents in the main a collection of "practically usable therapeutic methods and techniques." The book consists of a 'general' and a 'special' psychotherapy. In the first part, hypnotherapy, educational therapy, and psychoanalysis are considered; in the second part the therapy of different symptoms and the problem of 'abnormal constitutions' are discussed.

In contrast to Liébault, Bernheim, and Charcot, Isserlin distinguishes only two degrees or stages of hypnosis. Of considerable importance for therapy is his description of abnormal phenomena in the hypnotic state. A special effort is made to clarify the nature of suggestion. The psychologist will welcome the detailed account of the techniques of hypnotization. As regards educational therapy, the method of persuasion as used by Dubois and Déjérine receives due consideration. Volitional gymnastics, inhibition gymnastics, association therapy, psychomotor discipline, and occupational therapy are discussed. A good account of the development of the Freudian views and a critical analysis of these views are given. The conclusion is drawn that the method of association, the exploration of dreams and the exploration in the hypnotic state are practically usable methods.

This book is of special value to the physician who has to cope with problems of psychotherapy, and to anyone requiring information about modern psychotherapeutic procedures. The psychologist who considers an analysis of normal functions as basic for a therapy of abnormal and pathological phenomena will be somewhat disappointed. Isserlin starts from the techniques available. A book such as Kronfeld's psychotherapy is of greater interest to the psychologist since its point of departure is "the structure of personality, character, and disease picture." Although Isserlin does not present even the fundamentals of his medical psychology, his book contains a large number of psychologically interesting observations.

It is gratifying to note that the author does not overestimate the possibilities of psychotherapy since he considers the majority of mental disorders in this field as 'constitutional' diseases. Complete cure, therefore, can be effected in a limited number of cases.

Columbia University HEINRICH KLÜVER

The Ways of Knowing or The Methods of Philosophy. By Wm. Pepperell Montague. New York, The Macmillan Co., 1925. Pp. 427.

This is an essay in reconciliation, inspired as to method by Sidgwick's well-known *Methods of Ethics*, and as to content by the conviction that if "the methodological inquiry into the Ways of Knowing" were isolated from "the metaphysical inquiry into the Ways of Being" with which it has been confused in much of modern philosophy, and the extent of the latent agreement between rival views made clear, much time would be saved for the more serious business of philosophy (whose "primary interest is in the ways of things rather than in the ways of knowing them"), *i.e.* for metaphysics and theory of value.

Montague divided Methodology into Logic and Epistemology and his book into two corresponding parts: Part I, Ways of Attaining Knowledge—The Six Methods of Logic (Authoritarianism, Mysticism, Rationalism, Empiricism, Pragmatism, and Scepticism). Part II, Ways of Interpreting Knowledge—The Three Methods of Epistemology (Objectivism, Epistemological Dualism, and Subjectivism). He concludes each Part with a conciliatory chapter, attempting to show in the case of Part I that "The methods of logic, despite their differences, are capable of being combined in a federation in which each performs a more or less distinct and more or less valid and valuable function in the many-sided enterprise of attaining knowledge;" and in the case of Part II that "the three methods of epistemology, when reinterpreted in the manner explained in this chapter, are each of them true and each of them consistent with and supplementary of the others."

The last hundred pages of the book are occupied by a dialogue entitled "The Knower and the Known," one of the adroitest things of its kind in recent philosophical literature, in which Montague's essential argument is restated in simpler terms. Hylanous, a Realist, has no difficulty in showing Partridge, an Objectivist, Bryce, a Subjectivist, and Lovelace, a Dualist, that the truth of their respective epistemological positions is contained in his own very catholic one, and that "in the light of his eirenicon such concepts as truth and error can be translated without loss of meaning from any epistemology into any other."

Philosophers will quarrel with Professor Montague's stretching of the term 'logic,' reject his reinterpretations, and remain unconvinced by his reconcilations; but psychologists wishing to keep abreast of development in epistemology will find here a fairly complete marshalling of current views, and in the case of several of them expositions which have not been surpassed within the same compass.

Cornell University M. H. Fisch

Psychology in Business Relations. By A. J. Snow. Chicago, A. W Shaw Co., 1925. Pp. 554.

Snow attempts, as he states in his preface, to combine in one book the study of all the branches of business psychology for the benefit of the business man and the student of business. All discussion of the technicalities of psychology has been omitted. It is only fair to the author that this objective be kept in mind in surveying the book. One should remember, too, that writers of business books have a tendency to see the business man as a "tired business man" whose mental food must be palatable and not too heavy. Judging from business reviews the work has 'gone over' with its audience; and that is the measure of its success.

The author has chosen from the numerous sytems that one which best suited his purposes. The reader is introduced to this system in Chapter I, "The Fundamental Nature of Man." Just what the system is that has been chosen, the reviewer cannot say. He thought at first it was a clear-

cut behaviorism, but the frequent use of such concepts as feelings, desires, volitional, and rational actions, shows that such is not the case. The fact is that the most important concept that can be played upon in business psychology is 'motivation' of behavior, and for practical purposes it makes no difference whether one adopts the term instinct, desire, reflex chain, or any other as its mechanism. Snow does introduce the business man to the instinct controversy, and tells him that 'instinct' is a foolish notion, whereupon the acquaintance ceases.

The book is divided into five parts which deal with consumer, marketing, advertising, selling, and employment problems. The treatment of each topic is necessarily limited, and the material offers nothing of particular interest to the psychologist. Numerous references to psychological and business literature following each chapter would enable the business reader to study his subject more thoroughly if he should choose to do so.

Columbia University A. T. POFFENBERGER

A Manual of Individual Mental Tests and Testing. By AUGUSTA F. BRONNER, WILLIAM HEALY, GLADYS M. LOWE, MYRA S. SHIMBERG. Boston, Little, Brown & Co., 1927. Pp. x, 287.

This volume is classed as number four of the Judge Baker Foundation Publications. The manual had its growth in mental testing for the purpose of guidance in educational, personality, and conduct problems. The authors seek "to combat the uncritical acceptance of very narrow and simple measurements as offering adequate criteria for judgment of mental capacities," and urge the "great urgency for utilization of a much wider range of tests that the human individual may be better known in regard to his various mental capacities, in order to relate his abilities to his possible social, educational, vocational adjustments and achievements." Detailed account is given of those tests only that are not members of a scale. Mere mention is accorded 'omnibus tests,' educational, vocational, industrial, temperament tests and scales. Part Two is stated to include "every adequately standardized individual mental test." Full information is presented for the giving, scoring, and numerical evaluation of a large number of tests. The bibliography contains 319 titles.

The manual shows much diligence and is a handy volume which will be appreciated by anyone who gives it careful consideration.

University of Vermont PAUL C. SQUIRES

Problems of Philosophy. An Introductory Survey. By G. WATTS CUNNINGHAM. New York, Henry Holt & Co, 1924. Pp. xvii, 453.

The Principles and Problems of Philosophy. By ROY WOOD SELLARS. New York, The Macmillan Co., 1926. Pp. xiv, 517.

A little philosophy is a dangerous thing, especially if got in the usual 'introductory' course; and it may well be questioned whether the college undergraduate who has not time for the more leisurely and thorough introduction by way of logic, ethics, and the history of philosophy, might not be better off without the hasty and necessarily misleading survey which has become so popular in recent years. But if the promiscuous samplers who crowd our universities and colleges must continue to be given their nibble of philosophy along with all the other nibbles, there are probably no better textbooks for the purpose than the two before me. What is more to the point of their being noticed here, I know of none which would be more useful to the advanced student or younger teacher of psychology who has had no technical training in philosophy. They are of equal competence; the differences are in point of view and distribution of emphasis, and cannot be adequately indicated here. Labels are of doubtful value, but those which the authors would find least objectionable for their respective points

of view are, I take it, 'objective idealism' in the case of Professor Cunningham, 'evolutionary naturalism' in that of Professor Sellars. *The Principles and Problems of Philosophy* is "partly a revision, partly a supplementation," of Professor Sellars' earlier book, *The Essentials of Philosophy*, and, like it, follows the epistemological approach, tending to make central the problems presented by the natural sciences. Professor Cunningham's approach is more logical (in the Hegelian sense) and his emphasis more humanistic. But there is no dodging of important issues on the part of either. Of special interest to psychologists are Professor Sellars' chapter on "Psychology as a Natural Science" and Professor Cunningham's critical survey of theories of mind. Both books are equipped with abundant references to the literature of philosophy, and good indices.

Cornell University M. M. FISCH

Shell Shock and Its Aftermath. By NORMAN FENTON, with an introduction by Thomas W. Salmon. St. Louis, C. V. Mosby Co., 1926. Pp. 173.

Whoever wants information on any aspect of shell shock will do well to turn first to this volume. The available information on about every aspect of the problem is here systematically and pains-takingly summarized, with the addition of much new and valuable data. Statistical studies are reported on a very large number of topics including the proportions of officers and of men afflicted, the proportion of drafted *versus* volunteer soldiers involved, relation to different branches of the service, age distributions, personal and family histories, social and economic background, and relation to intelligence distribution. Within this small volume there are thirty-seven statistical tables, twelve graphs, and, as indicative of the literature examined, there are one hundred and seventy four footnote references. A notable addition to our knowledge is the author's extensive study of shell shock cases since the war, of their health and of what has has been done for them.

All this might lead the reader to expect some final statements concerning the nature of shell shock. But on this the author is very cautious. He doubts the sufficiency of the interpretations extant, and urges more consideration of shell shock in terms of the psychology of habit—especially because of the ease with which shell shock habits once established are found to be rearoused by any intensely emotional experience. He also urges more investigation of the shell shock by the technique of physiological chemistry before much faith is placed in any of the interpretation offered.

The reviewer turns from this volume with the wish that it were not quite so highly condensed, that it did not have quite so much the appearance of a doctoral dissertation. With so much knowledge at his command the author could have produced a more extended presentation which would have served a larger circle of readers than any existing volume on the subject, and which would in all probability have done more to stimulate the studies of psychoneurosis in industrial life which Dr. Salmon, who introduces the volume, properly hopes may be the consequence of the author's work.

University of Oregon EDMUND S. CONKLIN

Mental Tests, their History, Principles and Applications. By FRANK N. FREEMAN. Riverside Textbooks in Education, 1926. Pp. ix, 503.

The aim of this book is "to give an account of all the important types of mental tests." There is an historical organization of the general descriptive part of the text. The hope is expressed that the book may be of interest to laymen as well as to students of psychology and education.

The first part of the book is devoted to a description of tests, together with their valuation. Applications of mental tests are considered in the latter part of the work. The most significant applications are those in the field of scholastic problems, while next in importance are the applications of tests to vocational guidance and selection. The relation of mental tests to delinquency problems is considered. There is a discussion concerning the interpretation of differences between residential, occupational and racial groups, with the result that a 'middle-ground' is assumed by Professor Freeman.

Considerable attention is given to Spearman's theory. The closing chapter treats of the nature of intelligence. The author maintains that the Spearman 'G' is "unduly complex and rather difficult to conceive in its application." He holds that the hypothesis of the 'G' fails to fit any "known facts concerning the evolution of the intellect." Freeman proposes an hypothesis that is considered by him more adequately to fulfill the demands of the psychological description of intelligence and of the evolution of the brain. The position is taken that "the statistical evidence, so far as it goes, favors the notion that intelligence rests upon some more or less unitary factor." This book recommends itself to consideration as an introductory volume.

University of Vermont PAUL C. SQUIRES

The Psychology of Time. By MARY STURT. New York, Harcourt, Brace & Co., 1925. Pp. 150.

We find in this book a somewhat different approach to the study of time from that which we have learned to expect in experimental psychology. The author is chiefly concerned with the common-sense conception and with its development. She gives us a discussion of the evolution of the experience in the animal series, in the race, and in the individual. The growth of the concept in the child is examined with the aid of numerous test experiments.

The book will be disappointing to one who goes to it for a systematic review and critique of the experimental researches on the subject, or to one who seeks a discussion of modern metaphysical notions of time. The experimental work of Benussi in Germany and of Curtis in this country is neglected.

The chief merit of the book lies in the fact that it shows the importance of the genetic problem and that it suggests a new method of approach in the study of the time experience of the child. A book which summarizes and evaluates the experimental work has still to be written.

University of Kansas J. P. GUILFORD

Problems in Educational Administration. By GEORGE D. STRAYER, N. L. ENGELHARDT, and others. New York, Bureau of Publ., Teachers College, Columbia Univ., 1925. Pp. xvii, 755.

This volume applies to the field of educational administration, the so-called "case" method of instruction. Each problem describes a situation (usually an actual one), giving pertinent data and suggesting the specific questions involved. In this way the student is encouraged to begin his thinking in terms of actual conditions to be met, and from there to come, presumably, to the development of governing principles and a philosophy of administration. The chief danger in having a textbook devoted largely to a statement of problems is, obviously, that it may tend to crystallize the nature of the problems considered by the student.

The one hundred and sixteen problems in the book cover the important phases of school administration—national and state responsibility, local

administration, financing schools, business administration, school publicity, buildings and equipment, classification and progress of children, personnel management, and the like. While administrative techniques are emphasized, the implications of these in terms of a philosophy of administration are not overlooked. The various bibliographies give perhaps the best set of selected references on general administrative problems that has yet been published. No alert worker in school administration can afford to neglect this volume.

Cornell University — JULIAN E. BUTTERWORTH

A Short Outline of Comparative Psychology. By C. J. WARDEN. New York, W. W. Norton & Co., 1927. Pp. 96

Beginning with ancient lore, Dr. Warden, who writes from the animal laboratory of Columbia University, traces down to the present the history of the interpretation of animal behavior. He points out that pictures still extant suggest observation of animals as early as the Cro-Magnon period.

Dr. Warden's survey should provide profitable reading not only for biologists and psychologists but for all who wish to be well informed. The book, in addition to furnishing a background of fact, arouses curiosity concerning the investigations mentioned. References, however, are not given in the book. They may be obtained from the publishers who upon request will supply a general bibliography on the subject and a selected bibliography which corresponds to the five chapters of the book. These bibliographies, the reviewer feels, should be included in the book itself as they not only give the reader the source of the material, but add the weight of authority to the book.

A disproportionate amount of space is given to the work done at Columbia. The recent work of European comparative psychologists is ignored. The work of Köhler, for instance, is not mentioned. Except for the unexplanable omissions, this volume, the historical part of a forthcoming textbook, augers well for the complete book.

Cornell University — FLORENCE WOOLSEY HAZZARD

Religion in the Making. A. N. WHITEHEAD. New York, The Macmillan Co., 1926. Pp. 160.

This small book, made up of four Lowell Lectures for 1926, entitled respetively Religion in History, Religion and Dogma, Body and Spirit, Truth and Criticism, does not call for extensive review in a psychological journal. Its purpose is "to give a concise analysis of the various factors in human nature which go to form religion, to exhibit the inevitable transformation of religion with the transformation of knowledge, and more especially to direct attention to the foundation of religion on our apprehension of those permanent elements by reason of which there is a stable order in the world, permanent elements apart from which there could be no changing world."

A few of the many phrases by which Whitehead describes 'religion' may serve to give some idea of the direction taken by his thoughts: "religion is force of belief cleansing the inward parts;" "religion is the art and the theory of the internal life of man so far as it depends on the man himself and on what is permanent in the natures of things;" "religion is what man does with his solitariness;" "religion is world loyalty;" "God is the one systematic, complete fact, which is the antecedent ground conditioning every creative act."

Bryn Mawr College — JAMES H. LEUBA

Measurement in Secondary Education. By P. M. SYMONDS. New York, The Macmillan Co., 1927. Pp. xvii, 588.

As Symonds states it, the purpose of this volume is to bring together the material used in and derived from measurement in secondary schools, as well as to discuss the techniques employed. Inasmuch as this material and technique are spread through many periodicals, the volume makes them much more easily available for school men and students for whom the text is intended. The book describes in elementary fashion the standardized tests available in high school subjects, and for guidance in the clerical and mechanical vocations. Prediction of success in high school and college, ability grouping, tests of personality traits, and unreliability of marks all come in for some discussion.

The chapters on norms and standards, and how to handle the results of testing suffer from the defect common to many other discussions of similar topics: statistical terms such as coefficient of correlation, probable error, standard deviation, are arbitrarily used; but their significance and limitations are not clearly indicated. The danger here lies in the fact that misinterpretations and misuses might follow—a not infrequent occurrence in the past.

The author, however, has assembled valuable material which will be useful to the critical student.

Cornell University FRANK S. FREEMAN

Psychologie et Sociologie. By DANIEL ESSERTIER. Paris, Félix Alcan, 1927. Pp. 234.

This ambitious but well accomplished undertaking furnishes a guide to the vast literature that deals with the relation of psychology to sociology. The literature has been classified under five general headings. In the first are included periodicals, general treatises, text books and source books, all sociological in nature. The second contains a list of books illustrating the origin and evolution of the problem of the relation between the two sciences. The third enumerates books and articles dealing with questions of social psychology. In the fourth section, entitled "psychological sociology," are listed references to the topics of sociological interpretation of sentiments, intellectual functions and personality, while the fifth section is concerned with works on psychopathology, the rôle of instinct, and the psychology of value in relation to sociology.

The value of the bibliography is enhanced by short comments added to practically each one of the 622 books and articles listed, which either indicate the general position maintained by the author or briefly state the content. The importance of the book for sociologists lies in the fact that most of the listed writings approach the problem from the point of view of the methodological procedure in sociology. It should also be of interest to the psychologist, to whom it may offer a general orientation in the scope of psychological theory in the social sciences.

Essertier stresses the point throughout that the collaboration between psychology and sociology is not only useful and necessary but indispensable.

University of Illinois T. F. ABEL

The Seven Seals of Science. By JOSEPH MAYER. New York, The Century Co., 1927. Pp. xiv, 444.

This is a well-written, popular book on the history of science, a field where the lack of books makes this one welcome. The 'seven seals' are mathematics, astronomy, physics, chemistry, geology, biology, and psychology, and the author's thesis is that the development of the sciences

in some such order is the result of the unsealing of each by advances in those others that are necessarily its precursors. The author also hints that psychology may break an eighth seal for social science.

Dr. Mayer has written carefully and the result is very readable. He still speaks of the dark ages of science, though he shows some knowledge that Sarton has broken an historical seal, letting much light in upon and out from this hitherto obscure period.

It is interesting, of course, to find psychology admitted to the sisterhood of sciences. The forty-one pages on "modern psychology" are not, however, for the psychologist to read; the picture is too simple and the perspective too disputable. From James, according to the author, "the modern period in psychology may be said to have taken form," even though "Wundt is rightly called the father of modern psychology." The first section is almost entirely devoted to James. The second on experimental psychology, centers about Wundt. The third, on abnormal psychology, deals with Pinel, Mesmer, Coué, Charcot, Morton Prince, Janet and Freud. The fourth is on comparative psychology in the broad sense, and the last is on theory.

A psychologist might have written with more accurate perspective, but the fact remains that most psychologists do not write at this level and not many seem able to write so well.

E. G. B.

Thought and the Brain. By H. PIÉRON. English translation by C. K. Ogden. New York, Harcourt, Brace & Co., 1927. Pp. 255.

Piéron, in *Thought and the Brain*, puts in perspective the facts relevant to the most fundamental problems of the relation of mind and body. The book is divided into four sections.

The first part deals with the general conception of neural-mental functioning. The problem of the localization of cerebral function does not seem as well understood as the function of reflex organization. Piéron shows that neither the position that cerebral localization can be determined by the lesion of well-defined partial areas nor its opposite of general disturbance is comparable with the new facts, particularly those of war pathology. The function of the cortex is differentiated as (1) incito-associative receptor centers; (2) incito-motor centers; (3) receptive coördination centers; (4) incito-motor coördination centers. The fallacy of attempting to localize intelligence is shown by the multiplicity of possible pathways. The localization of faculties must give place to dynamic conceptions involving privileged synapses and variations within the sensory nuclei themselves.

The second part of the book deals especially with the incito-motor function of the brain. The elementary modes of sensory-motor thought are not confined to the cerebral cortex, but sub-cortical responses do not involve association. The principle of explaining physical processes by the idea of amoebial movements of the neuronic elements must give way to one that takes account of the functional individuality of the neurones themselves. The specific character of responses is explained by a non-technical exposition of the Chronaxy theory of Lapicque.

The third part concerns the cerebral mechanism of language and verbal thought. These involve a high degree of integration and include both cortical and sub-cortical mechanisms. It is the predominance of these centers in certain areas of the cerebral that explains the appearance of asphasic disturbances as resulting from limited lesions of the brain. Verbal localization is thus limited somewhat vaguely in the areas through which the great association pathways pass.

The fourth and final part of the book deals with the rôle and mechanism of affective regulation in mental life. The study of cerebral associative mechanisms, on which thought and perception depend, does not exhaust

the problems of mental activity. Mental activation is a function of an expenditure of nervous energy, the liberation of which is termed 'interest.' This varies with the predominant affective like tendency, which functions to partially canalize it. The facts now show that thalamic activation suffices to govern the general behavior of the organism, acting as a primitive mode of response to a situation.

In his treatment, Piéron gives much that is new, both factually and theoretically. Without bias in favor of any particular theory, the author confines himself to the examination of questions which can be stated with some precision in view of the scientific knowledge now at our disposal. The book is a fairly complete synthesis of European and American research, with the contributions of the war lesions to neurology and the latest work on asphasia receiving the major amount of attention. It is in keeping with the present dynamic trend of biological and psychological explanation, and will prove helpful to those interested in the correlation of mind and body.

Cornell University G. L. FREEMAN

Tests and Measurements in High School Instruction. By G. M. RICH and GEORGE D. STODDARD. New York and Chicago, World Book Co., 1927. Pp. xix, 381.

This book is essentially a resumé of existing tests and test methods in the field of high school instruction. The volume consists of four parts. The first part includes such topics as the historical background of measurement in secondary schools, the uses and limitations of various types of tests in the high school, and the criteria utilized in the selection of educational tests. The second part considers tests for such subjects as mathematics, the languages, science, and history. There is also included a discussion of mechanical and trade tests, commercial and music tests, survey tests, general intelligence tests and Junior high school tests. The third part deals with the informal objective examination methods, while the final part treats of the construction of educational and mental tests. Samples of topics in the fourth part are as follows: test validation, breaking a test into comparable forms, derivation of norms, determination of reliability, and perfecting the administration of a test.

This volume strikes a fair balance between theory and application. It is a most timely contribution in its special field. One of the attractive aspects of the work is to be found in those chapters that set forth principles of test construction. These chapters are of considerable assistance to the teacher, or whomever the person interested may be, in obtaining some insight into the principles involved in test construction. It may be said of this book that it is well adapted to serve as a stimulant toward the further solution of specific educational problems.

University of Vermont PAUL C. SQUIRES

Vorsatz, Wille, und Bedürfnis. Mit Vorbemerkungen über die psychischen Kräfte und Energien und die Struktur der Seele. By KURT LEWIN. Berlin, J. Springer, 1926. Pp. 92.

Lewin's study is a contribution to the psychology of affect and action. The first part brings some general remarks on the nature of psychic processes and on the problems involved in psychological experimentation; the second part deals with the facts and the theory of "intention, will, and need." The author advocates a sharp separation of "phenomenological" and "causal-dynamic" concepts. In other words: the results reached by objective observation and introspection, by the description of overt and implicit behavior, are not necessarily important in causal respect. Phenotypically, similar or identical situations or processes are not always equivalent from a causal point of view. It is only the recourse to the conditioning

factors which makes a phenotype scientifically meaningful. Stated succinctly, "introspectively" or "objectively" ascertained relations are not necessarily causal relations. This may be a commonplace to *Wissenschaftstheoretiker*, but by overlooking this fact a large number of controversies in psychology arise.

In considering the facts bearing on voluntary action, the author brings to light the "conditional-genetic" importance of the "*Quasibedürfnis.*" "*Quasibedürfnis*" and "situation" definitely determine a concrete psychological process in phenotypical respect. It is questionable whether a more detailed psychological analysis of *Bedürfnis* upon which the *Quasibedürfnis* is dependent would justify dismissing the question of value as Lewin does.

Columbia University — HEINRICH KLÜVER

Le Peyotl: La plante qui fait les yeux émerveillés. ALEXANDRE ROUHIER. Paris, G. Doin, 1927. Pp. xii, 371.

In this monograph, written by a Doctor of Pharmacy, will be found the most comprehensive study of peyote (mescal buttons, *Anhalonium Lewinii*) yet published. Not a great portion of the monograph is of general interest to psychologists but all of it would be invaluable to anyone working with the drug and its effects. Working from a pharmaceutical point of view, the author differentiates two subspecies of the plant. He advocates its therapeutic use as tone-sedative and as a stimulant of the nervous system.

Part I (pp. 3–86) is concerned with the geographical and botanical origins of Peyote. Part II (871–76) gives an outline of the history and ethnology of the drug. It is this and the next parts which are of most interest for psychologists. A good deal of space is given to descriptions of the peyote cults among different tribes of Mexican and American Indians. Although this material is not new, it is well summarized and is in a convenient form which should be of great interest to the social psychologist interested in religion and its development.

The last and longest part of the monograph (177–371) discusses the chemistry, pharmacology, and therapeutic possibilities of peyote. In this section, the author describes in some detail the 'visions' of some of the previous workers along with some new observations of his own. In these new observations, four of which are given in great detail, the author falls into the common error of over-emphasizing the visual reactions obtained with the use of the drug. Although it may be interesting to know what Havelock Ellis, Weir Mitchell, and now Rouhier see in the way of visions produced by ingesting peyote, such descriptions do not materially advance our knowledge of the effects of the drug. Certain physiological effects are also discussed. The reviewer regrets that these are not described with the amount of detail given to the 'visions.' Such a contribution would have been of great value.

The book contains a good many illustrations and a bibliography of 136 titles—which although large is far from complete.

University of Pennsylvania — SAMUEL W. FERNBERGER

Elements of Educational Psychology. By L. A. AVERILL. Riverside Textbooks in Education. New York, Houghton Mifflin Co., 1924. Pp. xii, 425.

This volume is arranged in 17 lessons, designed as a rather detailed and intensive work for teachers-in-training. Class experiments are introduced at the beginning of ten of the lessons, and lists of selected references and questions for discussion are appended to the different sections. The presentation is clear and informal, but occasionally it seems a bit wordy.

Cornell University — H. D. POWERS

NOTES AND DISCUSSIONS

Certain Points Concerning the Reliabilities of Experiments in Psychology

The purpose of this paper is (*1*) to give a short review of a controversy concerning the reliability of experiments in psychology which has injected itself into the literature during the last four years; and (*2*) against this background (*a*) to supplement the author's monograph on proofreader's illusions[1] with the employment of methods of calculation more accurate than certain of those which were published in 1924; (*b*) to demonstrate that our experiment as a whole was so reliable as to more than meet the statistical precepts of both Hunter and Kelley in the matter of differences between the various conditions of reading proof with all *Ss* treated as a single group; (*c*) to restrict certain conclusions in the monograph to the few cases of *Ss* involved; and (*d*) to offer the suggestion that, if our results were characterized by high reliability, that is, if one proofsheet can test an *S* as adequately as can five sheets, or if one series of sheets can satisfactorily replace four series, the method as employed and the sheets which were used give some hopeful promise of being useful in future vocational testing programs in the typography and journalism fields.

(*1*) Hunter and his students—Heron,[2] and Randolph[3]—and Davis and Tolman,[4] have demonstrated that the records of white rats in alternate runs in a maze do not give coefficients of high correlation and that the records of their running in two or more mazes show low coefficients of correlation. The correlations under consideration are employed as indices of the reliability of the experiment, and if high are thought to prove that the same ability is being tested. The values obtained by the above four investigators induced Hunter to suggest at the meeting of the A.P.A. in Washington, December, 1924, that all such maze studies are too unreliable for the measurement of individual differences, and of group differences as well. Carr, a pioneer in maze-investigations, took exception to Hunter's position, stating that, although his "hat was not satisfactory for eating, it still served a useful purpose" in covering his head, and that "maze-studies will continue at the University of Chicago." The discussion was continued in the literature by Carr[5] and Hunter,[6] the former saying that Hunter's criticisms of animal maze-studies are just as applicable to all other experiments in psychology as to the maze alone, and that the matter of 'unreliability' is a question of statistics, not of experimental technique. To this Hunter replied with a quotation from Kelley, to the effect that it is unwise to attempt, with a population as small as 30 *Ss*, a study of group differences unless a reliability is demonstrated to be as high as 0.4, and that with the same population individual differences should not be attempted unless the reliability is proven to be as high as 0.9. In the meantime, Murchison,[7] taking umbrage at the criticisms of Boring,[8] launched an attack upon

[1] H. R. Crosland, An investigation of proofreaders' illusions, *Univ. of Oregon Publ.*, 2, 1924, no. 6, 1-168.

[2] W. T. Heron and W. S. Hunter, Studies of the reliability of the problem box and the maze with human and animal subjects, *Comp. Psychol. Monog.*, 1, 1922, 1-56.

[3] W. S. Hunter and V. Randolph, Further studies on the reliability of the maze with rats and humans, *J. Comp. Psychol.*, 4, 1924, 431-442.

[4] F. C. Davis and E. C. Tolman, A note on the correlation of two mazes, *ibid.*, 4, 1924, 125-136.

[5] H. A. Carr, The reliability of the maze experiment, *ibid.*, 6, 1926, 85-94.

[6] W. S. Hunter, A reply to Professor Carr on the 'reliability of the maze experiment,' *ibid.*, 6, 1926, 393-398.

[7] Carl Murchison, An answer to Dr. Boring, this Journal, 37, 1926, 459-461.

[8] E. G. Boring, Scientific induction and statistics, *ibid.*, 303-307.

the various papers of the latter, his chief criticism being that Boring had employed too few cases as the basis for the rather general conclusions at which he had arrived. In the literature one meets the assumption frequently that the mere increase of the number of *Ss* tested will remove some of the unreliability of the experimental or testing technique. It is perhaps true that too few published experiments have been checked in order to show the reliability of the data, as a necessary step for determining whether or not the technique used was satisfactory or whether a much larger number of *Ss* should have been employed. Since the discussion has gained headway, Tryon[9] has shown that a difference which is empirically demonstrated between groups of *Ss* becomes all the more significant in proportion to the unreliability of the instrument or technique of measurement. Crum[10] and Holzinger[11] believe that they have demonstrated that adding more items to a test does not increase reliability to the degree which the Brown-Spearman prophecy formula would lead one to expect. And Lanier[12] has proved that the mere augmenting of the number of *Ss* engaged in an investigation by no means guarantees greater reliability, for, if the apparatus or technique is at fault, leading to variable results, multiplying these variations or errors by an increased number of *Ss* will in nowise raise the reliability of the experiment.

(*2*) (*a*). On pages 60-64 of our monograph we stated that a Pearson product-moment r was calculated for 5 samples, each considered by itself, of proofreading for each of 30 *Ss*, making in all 150 samples or items as a basis for the computation. Inasmuch as only 30 *Ss* instead of 150 took part in the experiment, it was bad statistical method to employ 5 samples from each person. The usual procedure would require that the mean of the 5 for each, 30 cases in all, be employed—a number of cases too small to suit the above formula. Since the publication of the monograph, we have employed in the three instances of Series A, B, and C the Pearson rank difference formula, obtaining correlation coefficients between mean accuracy and speed records as follows: Series A -0.55±0.09; Series B -0.43± 0.11; and Series C -0.42±0.11. It will be seen at a glance that these values do not vary greatly from those published in the monograph: -0.471±0.048; -0.457±0.046; and -0.478±0.045, respectively (pp. 61-63).

On pages 77-88 of our monograph the inaccuracy of reading proof was shown to increase progressively from the left to the right of the galley sheet, culminating in greatest error in the v-4 quarter column. The differences in the mean inaccuracy scores between v-1 and v-4, and v-3 and v-4, in all four series (A, B, C, and D) pooled together, for 26 *Ss*, have now been ascertained; these differences have been submitted to the D/σ diff. formula for the probability of the differences exceeding zero in many subsequent testings, were these repeated 99 times. The difference between the means for v-4 and v-1, throughout the series, is as significant as is shown by 98 chances in 100. That between the means for v-4 and v-3 is shown to be as significant as the probability of exceeding zero in 90 chances in 100.

Likewise, the difference in mean inaccuracy scores in Series A, B, and C, for the upper and lower halves of the proofsheets, 27 subjects, is found to be as significant as is indicated by 93 chances in 100. We have shown, furthermore (on pages 64-72 in the monograph), that the kind of error in the proofsheet has an influence on the inaccuracy with which the proof is read and corrected. Superfluous line spacing (SLS) and out-see-copy errors (OSC) were the kinds of mistakes which led to the most inaccurate

[9]R. Tryon, Effect of the unreliability of measurement on the difference between groups, *J. Comp. Psychol.*, 6, 1926, 449-453.

[10]W. L. Crum, *Amer. Math. Mo.*, 30, 1923, 296-301.

[11]K. J. Holzinger, Note on the use of Spearman's prophecy formula for reliability, *J. Educ. Psychol.*, 14, 1923, 302-305; cf. also K. J. Holzinger and Blythe Clayton, Further experiments on the applicability of Spearman's prophecy formula, *ibid.*, 16, 1925, 289-300.

[12]L. H. Lanier, *J. Exper. Psychol.*, 10, 1927, 69-113.

proofreading. Omitted inter-word spacing (OS), superfluous spacing (SS), and superfluous capitals (SC) were the errors least frequently left uncorrected by the proofreaders. That the difference between the most frequent and least frequent kinds of errors is significant, we have proven by the standard statistical procedure for that purpose; the difference between the scores for superfluous line spacing errors and superfluous capital errors is found to be as significant as to be expected to exceed zero in 100 cases out of 100 times. The smallest difference is that which exists between scores of misspellings and omitted paragraph spaces. This difference is found to be as significant as is indicated by 58 chances in 100, where in the formula we have included r, a correlation of +.43, between the two sets of scores.

(*b*) The reliability of the results which we obtained from our proofreaders, 30 in one series, 27 in two series, and 26 in the remaining series, we have now measured in three ways: (1) by comparing the scores on one sheet with the scores on another sheet in the same series to see if a high or low coefficient of correlation is obtained; in other words, to ascertain whether high or low variability was present among the *Ss* in the reading of sheets which were greatly alike and read under greatly similar conditions of purpose; (2) we have computed the reliability of the results between series, that is, by calculating the degree of correlation between the accuracy of reading a sheet in one series and the accuracy of reading proof on some sheet in another series; and (3) we have computed the inter-series correlation between the mean for five sheets of a series with the mean of five sheets of each of the other series, to ascertain whether or not a score earned in one series can prophesy the probable score in any other series. Such reliability calculations as we have mentioned above have been applied to speed scores as well as to inaccuracy scores. The correlation coefficients which were obtained under these three conditions are as follows.

(1) Intra-series correlation coefficients by individual sheets:[13]

INACCURACY SCORES

By percentages (30, 27, 27, and 26 *Ss*)

Series A, sheets 1, 2 = +0.919±0.02
Series B, sheets 1, 2 = +0.920±0.02
Series C, sheets 1, 2 = +0.929±0.02
Series D, sheets 1, 2 = +0.606±0.08

By absolute (Series D) scores (26*Ss*)

Sheets 1, 2 = +0.679±0.08	Sheets 2, 4 = +0.750±0.06
Sheets 1, 3 = +0.515±0.10	Sheets 2, 5 = +0.730±0.06
Sheets 1, 4 = +0.486±0.10	
Sheets 1, 5 = +0.440±0.11	Sheets 3, 4 = +0.617±0.08
	Sheets 3, 5 = +0.720±0.06
Sheets 2, 3 = +0.711±0.07	Sheets 4, 5 = +0.901±0.03

[13]In every case, r, was determined by Huffaker's method (*J. Educ. Psychol.*, 16, 1925, 265).

in which $r = \dfrac{\sigma_1 + \sigma_2 + (M_1 - M_2)^2 - \dfrac{\Sigma(X_1 - X_2)^2}{N}}{2\sigma_1 \cdot \sigma_2}$, when σ_1 and σ_2 are standard deviations of the two sets of scores; M_1 and M_2 are the means of the two sets of scores; x_1 and x_2 are the differences between the two sets, item by item in each set; and N is the number of cases (*Ss*).

SPEED SCORES (divided by 100)
By absolute time records in sec. (30, 27, and 27 *Ss*)
Series A, sheets 1, 2 = +0.947±0.01
Series B, sheets 1, 2 = +0.888±0.03
Series C, sheets 1, 2 = +0.950±0.01

(2) Cross-series correlation coefficients by individual sheets.

INACCURACY SCORES
By percentages (27 *Ss*)
Series A-1, B-1 = +0.860±0.04
Series A-1, C-1 = +0.610±0.08
Series B-1, C-1 = +0.910±0.02

SPEED SCORES (divided by 100)
By absolute time records in sec. (27 *Ss*)
Series A-1, B-1 = −0.250±0.12
Series A-1, C-1 = +0.210±0.12
Series B-1, C-1 = +0.680±0.07

(3) Cross-series correlation coefficients by means of series.

INACCURACY SCORES
By absolute scores (27 *Ss*)
Series A, B = +0.863±0.03
Series A, C = +0.939±0.02
Series A, D = +0.853±0.04
Series B, C = +0.904±0.03
Series B, D = +0.882±0.04
Series C, D = +0.881±0.03

SPEED SCORES (divided by 100)
By absolute time records in sec. (27 *Ss*)
Series A, B = −0.54±0.09
Series A, C = +0.17±0.13
Series B, C = +0.69±0.07

It is to be observed from the coefficients of correlation listed above that all values save three (those for A-1, B-1 and A-1, C-1, where speed r was −0.25 and +0.21 respectively, and for the A, C means, where speed r = +0.17) exceed the minimum of 0.40 which Kelley has set as a reliable basis for the study of group differences. Except for these three values, all coefficients exceed Kelley's minimum by large amounts. Had they been much smaller, such a fact would not have militated against the reliability of the experiment, as the original purpose proposed to discover real differences between the various series because of the divergent aims with which they were read and the differences existing between Series D and all other sheets. Moreover, had the above intra- or cross-series rs been much smaller, their smallness could then have been the basis for concluding that, inasmuch as the proofreading scores varied so widely in accuracy from sheet to sheet and from series to series, any adequate test of the abilities requires more than one sheet from each series and requires the entire four if not more series. The high rs shown above indicate, however, that no matter under what conditions reading is done, practically the same ability or abilities are being tested in the experiment. Perhaps this sweeping statement should be restricted to exclude sheets such as those which composed Series D, in which the greatest variations from sheet to sheet occurred. It will be noted above, moreover, that nine of the coefficients equal or exceed the minimum r of 0.90 which Kelley has proposed for an investigation of individual differences, while it must be re-

membered the minimum population for him is 30 *Ss*. It is true that in Series A we had employed the 30 necessary *Ss*, but in B, C, and D, only 27, 27, and 26 *Ss*, respectively, were used. Such individual differences as we have pointed out in the monograph (pp. 128-138) have been based upon still fewer cases, as for example the *Ss* composing the journalism, typography, psychology, and two student groups, comprising 5, 5, 5, 6, and 9 *Ss* respectively, with trained and untrained readers included among these *Ss*. The use of such small numbers of cases as the populations from which to draw conclusions concerning individual differences is obviously an insufficient basis for sweeping generalizations: this paper is written partly to limit our conclusions to the actual cases in hand. Nevertheless, practically all of the above coefficients of correlation indicate that the measuring device or technique employed was surprisingly accurate and reliable; such high reliability would seem to promise that future testing can with profit proceed along the lines which we have described in the monograph, and can have the added advantage of curtailing the 20 sheets to one or at most two test sheets, inasmuch as practically the same results can be prophesied because of the high r's exhibited in the foregoing table. To obtain results which have general applicability with respect to individual groups of *Ss* would require a great increase in the number of readers in each group or class.

Even the coefficients of correlation between the time scores, save in three instances, exceed the minimum of 0.40, a most surprising result, because one would imagine that the speed of reading proof would be far more variable than the accuracy of reading. That a negative relationship should occur between Series A and B was not unexpected; some indication of this fact was presented on pp. 34-42 and on p. 129 of the monograph. But that this r should be as large as −0.54 was a surprise. At several places in the monograph it was also stated that the reading of Series C resembled, in speed, Series B more than it did Series A; this fact has been confirmed by the above table of coefficients. The large value of r between B and C was unexpected.

If we arrange the above coefficients of correlation between mean accuracy scores for the series in descending order of size, we obtain the following sequence: A-C, B-C, C-D, A-B, A-D, and B-D. In this sequence it is obvious that, except for the A-B comparison, Series D is present in the comparisons which gave the lowest r's, that is, D correlated less highly with each of the other series than they correlated among themselves. The low correlation of D is probably to be explained by referring to four characteristics of the sheets of D which were absent from the other series. First, each sheet of D contained far fewer errors than the sheets of any other series, and it has been amply proven that the sheet containing the fewer errors is relatively more difficult to proofread accurately. Second, only two sheets of D contained the same number of errors, and the number of errors increased to the middle of the series; then followed a sheet of fewer errors and then one of a greater number of errors; in the three other series all sheets had approximately an equal number of objectively placed errors. Third, in D the reader was permitted to read and to correct proof in his own natural fashion, while in A, B, and C he was restrained by predetermined instructions, for accuracy, for speed, and for understanding, respectively. Fourth, the reading period in D was set at exactly 5 min., which fact led to unfinished and reread sheets, while no rereading and unlimited time were allowed in each of the other series. If one inspects the intra-series coefficients for Series D, above, it will be seen that high coefficients obtained when the compared sheets were greatly similar in number of objective errors, provided the sheets were closely adjacent or consecutive in order of presentation; see for example, the coefficients between D-2 and D-4 and between D-4 and D-5. This fact suggests in

the *Ss* a function of expectation which, if gratified, raised the values of the correlations but, if ungratified, lowered the values, presumably because such unfulfilled expectation brings out great variations among *Ss* in the efficiency of reading, some *Ss* being more affected than others. It also happens that the correlations between D-2 and D-4 and between D-4 and D-5 are the highest in Series D. Since the others are low, these high, and since D shows low correlation values with each of the other series, it appears to follow that the sheets of D are more adequate for the measurement of all aspects of proofreading ability than are the sheets of the other series. Therefore, if one cared to curtail our technique and to repeat the experiment with a saving of time and energy, sheet D-4 could conceivably serve the purpose. In fact, in a supplementary investigation, which is to be published elsewhere at an early date, we have employed D-4 as a means of testing all-around proofreading ability, along with measurements of the range of visual perceptual apprehension. Where we have defined proofreading ability as being the product of reading time and reading accuracy, we have obtained a +0.84 correlation with range of apprehension scores, this value being higher than many of the values which are used in predicting college success from intelligence or high school record scores.

(*c*) In view of Murchison's criticisms of Boring and in view of Kelley's precepts for the study of individual differences, certain conclusions appearing on pp. 128-138 of the monograph should be restricted to the *Ss* who served in the investigation. These conclusions are as follows: (1) Several readers untrained in proofreading were more accurate than the trained *Ss*. (2) Speedy reading was, for the most part, characteristic of the untrained persons. (3) Slow reading did not proportionately increase the accuracy of the trained readers. (4) Individual differences existed in our *Ss* with respect to susceptibility to the number of errors objectively present in the sheets. (5) Slight individual differences in susceptibility to various kinds of errors were indicated. (6) The trained and untrained *Ss* differed as regards the effects of different purposes in reading on both time and accuracy scores. (7) Several statements were made concerning differences between groups of readers, for example, between journalists and psychologists, between both of these and students, between job-shop typographers and news typographers, etc. At the time of the preparation of the monograph it was realized that too few cases were included in each group so that sweeping generalizations were out of order. All conclusions other than those listed above were based upon data characteristic of true group differences—the group having not fewer than 26 *Ss* at any one time; and thus are in accord with the exacting standards of Kelley, Hunter, and others.

(*d*) If the results of the investigation of proofreaders' illusions possess the high reliability which is indicated by this paper, meeting so well the standards of statisticians by revealing only very small variations in accuracy scores from sheet to sheet and from series to series, so that one can feel that real proofreading ability has been measured, any program of vocational testing in the fields of journalism and typography cannot well ignore and may adopt with advantage the technique which we have worked out and have described at great length. Much of our labor can be avoided, it appears, by the curtailing of the number of sheets employed and by using only one general condition of purpose for the reading that is required of each person tested. Moreover, if it should turn out,[14] as has been tentatively suggested by both Downey[15] and Sterzinger,[16] that a high correlation

[14] We are planning an investigation of this question which we hope will be settled unequivocally.

[15] J. E. Downey, The proof-reader's illusion and general intelligence, *J. Phil., Psychol., & Sci. Method*, 15, 1918, 44-47.

[16] Othmar Sterzinger, Zur Prüfung und Untersuchung der abstrakten Aufmerksamkeit, *Zsch. f. angew. Psychol.*, 23, 1924, 121-161.

obtains between proofreading ability and general intelligence, a foreshortened set of proofsheets, much like our own, conceivably could be introduced with advantage into an intelligence measurement program.

Summary. This paper may be taken as a full and honest confession that the author is no more than a novice at statistical procedure, a fact, however which does not prevent him from sympathizing greatly with Boring in the belief that it is possible to test with statistical correctness large numbers of *Ss* and obtain differences whose probability of being real differences is great, while such differences may be ridiculously untrue; with Lanier in the belief that the mere possession of a population large enough to be statistically acceptable is not necessarily a guarantee of results more reliable than those obtained from fewer cases; with Boring and Carr in the belief that 'reliability' is more a matter of statistical precept than it is of the validity of data or of the effectiveness of technique; with Carr, perhaps, that not every investigation undertaken by statistical novices can be proven, *post hoc*, to have been so unreliable as not to warrant notice and acceptance; and with Tryon that differences arising from a clumsy or unreliable technique are, by reason of that very fact, all the more surprising and significant. When the author planned the investigation of proofreading he knew nothing of statistical precepts and methodology; now, due to the help of colleagues, he knows a little more, although he is still a novice. The computations above have been undertaken through curiosity as to what would be revealed if acceptable statistical methods were employed and through the desire to know if the investigation must necessarily be unacceptable merely because the investigator had proceeded without the employment of statistics '*à la* Hoyle.' The author has satisfied himself; it is hoped that the reader is equally satisfied.

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DR. WEVER ON ATTENTION AND CLEARNESS

In the January number of the JOURNAL Dr. Wever[1] reports a study in which he criticizes the concept of attributive clearness in general and my work on *Attributive vs. Cognitive Clearness*[2] in particular. Dr. Wever's study was seriously undertaken and it therefore deserves a serious reply. My one regret, however, is that Dr. Wever did not make as earnest an attempt to understand as he did to criticize.

Dr. Wever's difficulties are for the most part of his own making. Before criticizing one should understand, and this obviously Dr. Wever has failed to do. In fairness to Dr. Wever, however, it must be said that the Titchenerian doctrine of attention is difficult, and that it has been subject since its inception to much misunderstanding and misinterpretation[3]—so he is

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[1] E. G. Wever, Attention and clearness in the perception of figure and ground, this JOURNAL, 40, 1928, 51-74.

[2] K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 183-230.

[3] I list here the criticisms that have been made of the Titchenerian doctrine and of the experimental work that was done under it. I give also the replies that have been made in rebuttal. The criticisms, as the replies show, rest upon misconceptions of the doctrine.

H. R. Marshall, Clearness, intensity, and attention, *J. Phil., Psychol., & Sci. Methods*, 6, 1909, 287-290.

M. W. Calkins, The abandonment of sensationalism in psychology, this JOURNAL, 20, 1909, 287.

R. S. Woodworth, Review of Titchener's 'Text-book of psychology,' *J. Phil., Psychol., & Sci. Methods*, 6, 190, 692 ff.

Reply: E. B. Titchener, Attention as sensory clearness, *ibid.*, 7, 1910, 180-182.

F. Hillebrand, Review of Titchener's 'Lectures on the elementary psychology of feeling and attention,' *Zsch. f. Psychol.*, 58, 1910, 141-149.

C. Rahn, The relation of sensation to other categories in contemporary psychology, *Psychol. Monog.*, 16, 1913, no. 67, 29 ff.

(*Footnote 3 continued on page 338*)

in good company. The difficulties that many have with the Titchenerian doctrine are, I firmly believe, due in a large part to a terminological confusion occasioned by Titchener's unfortunate use of the term 'clearness.'[4]

Titchener found in his examination of the works of various writers on attention, "that, wherever you look you find some form of reference to clearness; clearness is, so to say the first thing that men lay hands on, when they begin to speak about attention."[5] Clearness, or some form of reference to clearness is the common denominator of the accounts of attention. It was this fact that led Titchener to the consideration of 'clearness' as an attribute of sensation, and to his doctrine of attention. He says, upon this point, "it seems to me beyond question that the problem of attention centers in the fact of sensible clearness."[6] Titchener found that 'clearness' fulfilled the qualifications of an attribute,[7] and, after some hesitation between the terms 'vividness' and 'clearness,'[8] he chose the latter to designate that aspect of experience by which the focal processes are distinguished from the marginal. 'Clearness' was an unfortunate choice, as Titchener himself later realized, for he changed the name of this aspect to 'vividness' in 1915,[9] and again to 'attensity' in 1924. The word 'attensity' was coined, Titchener says, "to replace what is ordinarily called sensory or attributive clearness or vividness, in the hope that . . . its freedom from associations may help to clear up the confusion of cognitive and attributive clearness."[10]

Had Titchener employed the term 'vividness' (or better yet the term 'attensity') when he formulated his theory we should, I believe, have been spared a deal of controversy and confusion, for 'clearness' has proved to be a very slippery term, particularly when applied to visual experience—the modality with which Dr. Wever was concerned. Four kinds of clearnesses may be involved in a visual experience: (1) attributive clearness, *i.e.* the

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Reply: Titchener, Sensation and system, this JOURNAL, 26, 1915, 258-267.

C. A. Brits, *Eine theoretische und experimentelle Untersuchung über den psychologischen Begriff der Klarheit*, 1913.

Reply: Titchener, The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 43-61; K. M. Dallenbach, Attributive *vs.* cognitive clearness, *J. Exper. Psychol.*, 3, 1920, 183-230.

K. Koffka, Review of Dallenbach's 'The Measurement of Attention,' *Zsch. f. Psychol.*, 73, 1915, 281 f.

Reply: Dallenbach, Note of the experimental study of attention, this JOURNAL, 31, 1920, 215.

J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 1916, no. 35, 1 ff.

Reply: Dallenbach, Dr. Morgan on the measurement of attention, this JOURNAL, 29, 1918, 122 f.

H. Henning, Review of Dallenbach's The measurement of attention in the field of cutaneous sensation,' *Zsch. f. Psychol.*, 82, 1919, 245.

Reply: Dallenbach, *op. cit.*, this JOURNAL, 29, 1918, 122 f.

H. M. Johnson, Reaction-time measurements, *Psychol. Bull.*, 20, 1923, 577-580.

Reply: Dallenbach, Dr. Johnson on reaction-time experiments, this JOURNAL, 35, 1924, 305-307.

H. S. Oberly, The range for visual attention, cognition, and apprehension, this JOURNAL, 35, 1924, 332-352.

Reply: Dallenbach, Dr. Oberly on 'The range for visual attention, cognition and apprehension,' *ibid.*, 36, 1925, 154-156.

H. M. Johnson, Definition and measurement of attention, *ibid.*, 36, 1925, 601-615.

Reply: Dallenbach, Dr. Johnson on the measurement of attention, *ibid.*, 37, 1926, 149-154.

S. W. Fernberger, The 'range of attention' experiment, *ibid.*, 38, 1927, 478-479.

Reply: Dallenbach, Dr. Fernberger, on the 'range of attention' experiment, *ibid.*, 38, 1927, 479-481.

[4]E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 171-317.

[5]*Ibid.*, 186.

[6]*Ibid.*, 182.

[7]*Ibid.*, 211-220; cf. also The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 53 f.

[8]*Ibid.*, *Psychol. Rev.*, 24, 1917, 55.

[9]E. B. Titchener, *A Beginner's Psychology*, 66, 92 ff.

[10]E. B. Titchener, The term 'attensity,' this JOURNAL, 35, 1924, 156.

visual experiences may occupy the focus or the margin of attention; (2) cognitive clearness,[11] *i.e.* the visual experiences may or may not be cognized; (3) physiological clearness, *i.e.* the visual experiences, due to the difference in sensitivity of the retina, may be focal and distinct, or peripheral and at varying degrees of indistinctness; (4) physical clearness, *i.e.* the visual image, due to spherical aberration of the crystalline lens, may be sharp and definite, or diffuse and vague.

These 'clearnesses' may vary concomitantly or—except for physical and physiological 'clearness'—independently. I do not know for certain, but I think that physical and physiological clearness always vary concomitantly, *i.e.* they are tied by the structure of the lens and retina. However that may be, it is certain that attributive and cognitive clearness may vary independently of them and of each other. That attributive and cognitive clearness may vary independently of physical and physiological clearness is attested by the fact that an impression in the periphery of vision may be cognized and attended to. That they may vary independently of each other was shown by the results of my study in which it was discovered that "an impression may be attributively clear and cognitively clear; attributively clear and cognitively unclear; attributively unclear and cognitively clear; attributively unclear and cognitively unclear."[12] These findings have recently been confirmed by Cooper who found that "with attributive clearness high or low may be combined either high or low cognitive clearness."[13]

All of these 'clearnesses,' with the possible exception of attributive, are easy to comprehend. Attributive clearness is more difficult than the others simply because it is an ultimate. Titchener, in his reply to Britz's criticism, writes that very little can be said about the actual nature of attributive clearness: "You cannot say much about a thing you regard as ultimate to your science; any attempt at a definition runs over, by force of circumstances, into what Wundt would call *tautologische Umschreibung*. I have had recourse to a number of these periphrases; but I have tried above all, to exhibit the thing itself, to state conditions under which it may be experienced and identified in experience. Quality and intensity are here in the same box with clearness. You can exhibit qualities or intensities, as you can exhibit clearnesses; but when you attempt to define them, you find yourself talking round them. If Britz had performed Geissler's simple experiment[14] with the two metronomes, equated for quality and intensity of sound, he would have discovered at first hand what I mean by sensory or attributive clearness."[15]

Instead of performing the experiments that exhibit attributive clearness, the critics have jumped at conclusions. They have thought that they understood but they have, without exception, mistaken one or more of these other kinds of 'clearnesses' (most frequently cognitive clearness) for the phenomenon in question. Now my point is simply this: had the term 'attensity'—or any other term besides 'clearness'—been used by Titchener, confusion would have been less likely. Not that it would have made the critics sagacious, but that it would have had a tendency to

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[11]For a differentiation of attributive and cognitive clearness see Titchener, *Feeling and Attention*, 238 f.; *Experimental Psychology of the Thought Processes*, 1909, 17; S. S. Colvin, *The Learning Process*, 1911, 255 f.; and Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 199-225.

[12]K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 229.

[13]S. F. Cooper, The effect of brightness in the range of attention experiment, this JOURNAL, 40, 1928, 268.

[14]L. R. Geissler, The measurement of attention, *ibid.*, 20, 1909, 510; cf. also K. M. Dallenbach, The measurement of attention, *ibid.*, 24, 1913, 467.

[15]E. B. Titchener, The psychological concept of clearness, *Psychol. Rev.*, 24, 1917, 55 f.

keep them from thinking that they understood what was meant when, as their criticisms show, they did not. That alone would have been a gain.

In the visual experiences of every day life all four of the 'clearnesses' may exist together, and even a practiced observer may find it difficult to differentiate between them. Vision therefore is a particularly troublesome sense department in which to work. The fields of audition or of cutaneous sensitivity are much simpler. But if one must work in vision one should at least make the introspective task as simple as possible by restricting the sensory field—as Britz and I did with the telescope[16]—so that visual experience is limited to the foveal regions of the retina, and physical and physiological clearnesses are eliminated. In any case, whether one works with the restricted visual field, with audition, or with touch, the investigation of clearness—since one is dealing with an ultimate—should be held to the sensory level. Dr. Wever agrees in theory that "the problem should be reduced to its simplest terms,"[17] but he works in vision—the most complex of the sense departments; he does nothing to restrict his visual field—all four 'clearnesses' are effective in his experiments; and he presents his observers a perceptual task! It is small wonder therefore that he obtained nothing but an undifferentiated clearness. Dr. Wever was ill advised, he 'does not know his clearnesses.'

So much by way of general reply; I now turn to the particulars of Dr. Wever's criticism. To the charge of misunderstanding must now be added the charges of misreading, misinterpreting, and misquoting. I give instances.

(1) Dr. Wever says, "Titchener's distinction of sensory and cognitive clearness was given experimental confirmation in a study by Dallenbach, a study *expressly devised* to *substantiate* the concept of sensory clearness in the face of a *severe* attack by Britz."[18] Dr. Wever should know that experiments are undertaken to test hypotheses not to "substantiate" them. But in any case my experiments had a different purpose. They were undertaken to test Britz's results—as even a superficial reading of my study should have shown him, for I say in the introduction, "the present paper reports the results of a series of experiments carried out so far as possible under the conditions laid down by Britz."[19] I can assure Dr. Wever that the results of my study would have been reported whatever their outcome. The experiments, moreover, were of Britz's 'devising' not mine, as Dr. Wever should have learned from my paper even though he had not read Britz. "Severe," furthermore, is hardly the word that one, who has critically read Britz or Titchener's reply, would use to characterize the "attack." But as incredible as it may seem, we have no evidence that Dr. Wever has read either.[20]

(2) The reader of Dr. Wever's paper who has not read mine would hardly guess that his principal criticisms[21] against my experiments were practically quotations from my paper. I devoted a section to the discussions of the "complexity of the problem;"[22] I pointed out "that neither the method nor the instruction was conducive to good psychological description. The observers were required to note meaning and process and to give a full report of both. An extremely difficult instruction!"[23] I well recog-

[16] K. M. Dallenbach, *op. cit.*, *J. Exper. Psychol.*, 3, 1920, 186.
[17] E. G. Wever, *op. cit.*, 54.
[18] *Op. cit.*, 53. Italics mine.
[19] *Op. cit.*, 184.
[20] Dr. Wever gives the page and sectional references to every article that he cites in footnote with the single exception of Britz's study—and Britz devotes only a part of his paper to the Titchenerian concept. The first part of the paper is devoted to Wundt and the last part to the report of the experiments. Dr. Wever, furthermore, makes no reference to Titchener's reply to Britz. Dr. Wever should have known of it for it is the first reference that I give in my study.
[21] *Op. cit.*, 53, 68 ff. [22] *Op. cit.*, 196-198. [23] *Op. cit.*, 196.

nized the defects of the method but I had no choice in the matter. I was repeating Britz and any change in the method or procedure would have invalidated the comparison of my results with his. With all its faults, however, Britz's method and procedure were superior to Dr. Wever's. Britz at least eliminated physical and physiological clearness from his experiments, and he thought to work upon the sensory level. Indeed, if Dr. Wever had attentively read Britz he would have noted his strictures against working upon this problem at the perceptual level.

(3) Dr. Wever's quotations from my paper are, furthermore, misleading to a reader who does not have my report clearly in mind. He says, for example, that one of my observers "maintained that 'the report is so complicated that it seems foolish to talk about attributive clearness.' "[24] What I said was, "W says in one of the early reports: 'The report is so complicated that it seems foolish to talk about attributive clearness.' "[25] The implications of Dr. Wever's quotation is very different from my statement. The qualification "in one of the early reports" is not without significance.

(4) I am said by Dr. Wever to rely in my discussion upon two premises: "the first is that the *O* is able to report upon the clearness of a given part-content accordingly as it stands out to his grasp in the total experimental content; and the second is the clearness of conscious processes determines the fullness of report upon these processes."[26] I nowhere formulate these premises; they are of Dr. Wever's own making. I relied entirely upon the reports of my observers. I am willing, however, if I understand them correctly, to accept the first—I believe that an observer can tell which processes he is attending to and which he is attending from; and I accept the second if Dr. Wever's "clearness" means cognitive clearness, but I must emphatically deny it if Dr. Wever's "clearness" means attributive clearness for the results of my study and Cooper's[27] (made independently and in a different laboratory so 'atmosphere' had nothing to do with his results) stand in direct opposition to it. My results show (1) that attributive and cognitive clearness each has its own conditions, and (2) that attributive clearness is one of the conditions of cognitive clearness, but it is only one of 12 conditions that obtained in my experiments.[28] It is not a compulsory condition, it operates for cognitive clearness in variation with the other 11 as, for example, the factors of chance disposition act for or against the perception of duality in the psychophysical experiment on the two-point limen. Impressions are cognized when the conditions are 'adequate,' and the degree of cognition varies with the 'grade of chance disposition.'

(5) Dr. Wever shows from my data that "there exists a high correlation between 'levels of attributive clearness' and 'degrees of cognition.' "[29] Since attributive clearness is one of the conditions of cognition we should expect that relation to exist. He will also find a high correlation between every one of the other 11 conditions of cognitive clearness and degrees of cognition if he will but take the trouble to look. The important point in this connection, however, is not that there is a high correlation between the two clearnesses, but that there is not an invariable correlation—which we should expect if there were but one kind of clearness. "But why," Dr. Wever asks, "should the fact that these two things do not give a one-to-one correlation be any proof that the observer has had within his consciousness two types of clearness?"[30] Well, the observers reported the two kinds of clearness within a given consciousness. When competent observers report that the impressions of a given moment are attributively clear and cognitively unclear, and repeatedly give such reports through the course of 240 experiments I am inclined to accept them.

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[24] *Op. cit.*, 68.
[25] *Op. cit.*, 198.
[26] *Op. cit.*, 69.
[27] *Op. cit.*
[28] *Op. cit.*, 213-215.
[29] *Op. cit.*, 70.
[30] *Op. cit.*, 69.

(6) Dr. Wever's method is as, I have already pointed out, inadequate to the problem. Still it is possible even in his method, as bad as it is, to differentiate in the excerpts that he gives of his observers introspections, the various kinds of clearnesses that may be integrated in the perception of a total visual field. I give examples: B (p. 66) "An unclear field is 'fuzzy.' An unclear field is on the fringe of the picture" [physical and physiological clearness]. Z (p. 67) "A thing is clear in proportion as it can be reported upon" [cognitive clearness]. Z (p. 67) "In the experimental field one part of the field will be distinct, and will seem to stand out more, is more *eindringlich*, like the focus as opposed to the margin" [attributive clearness, I cannot of course be certain because this excerpt appears out of its context].

(7) Since I regard Dr. Wever's method as inadequate and his "undifferentiated clearness" as a composite, it is perhaps unnecessary for me to say that I consider his discussion of the law of two-levels as irrelevant. It is, however, a very significant fact that the investigators who thought they had demonstrated the multi-level type were all working in vision, with an unrestricted field where physical and physiological clearness had an opportunity to be effective.

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THE ACCURACY OF MÜLLER-URBAN'S METHOD OF CONSTANT STIMULI

In view of Dr. Lufkin's note,[1] let me try to clear what remained obscure in my reply[2] to Dr. Urban.[3] Being purely a *pièce d'occasion* and by no means a review of the whole problem, my first paper had but one aim: to demonstrate beyond reach of cavil that Urban's formula (probable error of the constant-stimulus limen) and his own figures are in error; theory of necessity therefore made way to factual proof. We may now enquire in more detail how c and h are related.

Consider the equations

$$c - hJ_1 + \gamma_1 = \rho_1, \text{ of weight } P_1;$$
$$c - hJ_2 + \gamma_2 = \rho_2, \text{ of weight } P_2;$$
$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$
$$c - hJ_n + \gamma_n = \rho_n, \text{ of weight } P_n;$$

wherein h is the 'precision;' c equals ah (a being the limen); J_k is the stimulus judicandus to be compared with a standard S; ρ_k is a residual (amount by which left member of its equation departs from zero); P_k is relative weight of the observation-equation k in deriving the normal equations for c and h. The Müller-Urban technique, as we know, is a device for minimizing the sum of these residua when squared ($\rho^2_1 + \rho^2_2 + \ldots + \rho^2_n$) and duly weighted; it thereby enables us to lay through the observed percentages an ideal curve, Φ (γ), whose course will deviate as little as possible from the empirical figures.

The question is: in what conditions and to what degree will these *minimizing* values of h and c correlate with each other? Suppose we make many determinations of the limen and thereby locate n points (c_1h_1, c_2h_2, . . . , c_nh_n) where $\Sigma\rho^2$ is least; these n minima, instead of scattering uniformly, will cluster in some part of the c-h plane. The cluster reveals, by its form and orientation to the coördinate-axes, in what measure c and h are related. A correlation-surface is commonly made up by erecting, in each square of the double-entry field (x, y), a pillar whose height equals the frequency of that combination (x_k, y_k); let us build our model instead

[1] H. M. Lufkin, Accuracy of the method of constant stimuli, this JOURNAL, 38, 1927, 666-7.

[2] Elmer Culler, Accuracy of the method of constant stimuli: a reply to Dr. Urban, this JOURNAL, 38, 1927, 307-12.

[3] F. M. Urban, Accuracy of the method of constant stimuli, *ibid.*, 38, 1927, 252-56.

by summing $\Sigma\rho^2$ for all pairs lying in a given square of the c-h plane and then raising an ordinate equal to this total. At h = 0.100, c = 11.0 are, let us say, 15 of our n cases; instead of placing as usual a column of length 15, we sum their $\Sigma\rho^2$ ($\Sigma\rho^2_1 + \Sigma\rho^2_2 + \ldots + \Sigma\rho^2_{15}$) and embody this total in the ordinate. Our solid model thus incorporates, not n cases all of unit value, but n $\Sigma\rho^2$, unequal in magnitude but alike in being minimal each for its own series. Every ordinate now symbolizes an equation of the form[4] (after P is multiplied in)

$$[P\rho^2] = [PJ^2]h^2 - 2[PJ]hc + c^2 - 2[PJ\gamma]h + 2[P\gamma]c + [P\gamma^2].$$

By writing $[P\rho^2] = k$, we automatically pick those ordinates which are equal, as though by slicing the top off our model on a plane k units above the c-h base; the edge of this cut is a contour-line of our solid cluster. Being of second degree and of type

$$Ax^2 + 2Bxy + Cy^2 + 2Dx + 2Ey + F = 0,$$

this contour-equation stands for a conic section (ellipse, parabola, hyperbola, with their limiting and special forms).[5] Thomson says indeed that it "represents a family of ellipses."[6] This is not wholly true; the general equation always describes a conic; if we are to define it more precisely, other conditions need be known, as arrayed in the schedule below.

Condition[7]	Type of conic
I. $\Delta \neq 0$	
1. $B^2 > AC$	hyperbola
2. $B^2 = AC$	parabola
3. $B^2 < AC$	ellipse
a. $B = 0, A = C$	circle
II. $\Delta = 0$	two straight lines, real or imaginary

Until we know the value of Δ and of $(B^2 - AC)$, no more can be said about the contour of our model. In practice however it is almost sure to be elliptical (with a straight line as limiting form) or possibly hyperbolic. So long as the correlation-figure (ellipse) runs obliquely athwart the c- and h-axes, these variables are bound to correlate in some degree; only if major and minor axes parallel the coördinates will r_{ch} become nil. The model's obliquity, Θ (angle of major axis with x-coördinate) is measured by the relation

$$\tan 2\Theta = \frac{B}{A - 0}.$$

When $B = 0$, $\Theta = 0°$ or $90°$; in either case, $r = 0$; only if the term in xy vanish, will r itself disappear. Since B (coefficient of the xy-term) = [PJ], B depends directly on magnitude of J and site of the origin. The whole c-h model then, in form and orientation alike, may be completely altered by merely moving the point of reference; the size of r can be varied at will by simply changing J (or the limen a; that value of J where the proportion of comparative judgments—heavier, warmer—is 50% of the whole).

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[4] The square brackets are signs of summation.

[5] The propositions here stated are proved in analytical geometry; these books may be consulted:

A. C. Jones, *Introduction to Algebraical Geometry*, 1912, 226.
E. H. Askwith, *Analytical Geometry of the Conic Sections*, 1908, 120.
S. L. Loney, *Elements of Coördinate Geometry*, 1912, 330.
C. H. Ashton, *Analytic Geometry*, 1900, 166.

[6] The second term in his equation should be multiplied by two. Cf. G. H. Thomson, Accuracy of the Φ (γ) process, *Brit. J. Psychol.*, 7, 1914, 54.

[7] The determinant, Δ, equals

$$\begin{vmatrix} A, & B, & D \\ B, & C, & E \\ D, & E, & F \end{vmatrix} = ACF + 2BDE - AE^2 - CD^2 - FB^2.$$

The following schema may do to show how 'tricky' is the h-c relation and how carefully it must be handled.

	h =	.10	.20	.30	.40	.50	
Origin at 0	a =	80	90	100	110	120	
	c =	8	18	30	44	60	r_{ch} = .996
Origin at 130	a =	−50	−40	−30	−20	−10	
	c =	−5	−8	−9	−8	−5	r_{ch} = .000

The limen, a, is taken once from zero, then from 130; this purely formal change reduces r from 0.996 to 0.000. The focus of trouble resides in the product c (=ah); any change in one of its factors disturbs the whole c-h correlation-surface. In correlating a product with one of its own factors (ah with h), we are dealing with a shifty relation.

We are now prepared to scan Dr. Lufkin's comment on my reply to Dr. Urban; two criticisms, if I read correctly, are implied in his note.

(1) By using ρ instead of r for measuring their concomitance, I exaggerated the amount of relation between h and c. In reply, I was concerned only with proving a high positive correlation of h and c in Urban's data; its exact magnitude for my purpose was wholly incidental. Ease and speed of computation were my only reasons for using ρ; neither then nor since have I computed r for those figures. We all know r to be a more precise measure than ρ; but with short series[8] no index of relation is highly reliable. Consider r and ρ in Dr. Lufkin's own figures;[9] the means run as follows:

	S_1		S_2	
	r	ρ	r	ρ
Lifted weights	.798	.732	.952	.875
Visual apprehension	.595	.750	.538	.700

In two cases r exceeds ρ, in two, ρ exceeds r; neither is uniformly higher than the other. The correlation of h and c in Urban's data is so high as to be unquestionably significant; this we believe no one will venture to deny; if then r be a few points higher or lower than ρ, what of it?

(2) If c be a direct function of h [c = F(h)], then the method of least squares cannot be applied and the whole $\Phi(\gamma)$ procedure must be abandoned. In my former article, let me say, I sought to show that c and h are de facto dependent in Urban's data, as proved by their high covariance; *not* mathematically dependent, as though either were uniquely determined by the other. The equation, c = F(h), whose implication Dr. Lufkin traces through, is far too simple; c = F(h, . . . , m, n, o, . . .) is nearer the truth. The magnitude of both c and h is affected, in variable degree, by other determinants as well. In a word there is no consistent or inherent relation of c and h; their r may range from zero to unity; but even if the correlation be casual instead of necessary, it vitiates Urban's formula all the same. The phrase "functional dependence" perhaps gave a wrong impression of my view.[10] To Dr. Lufkin it may well have meant mathematically functional (any value of c immutably fixes the corresponding value of h). The term was here used instead to mean the kind of empirical dependence which is found in natural phenomena: and not the 'structural' or necessary relations of theory. This meaning of the word functional is surely implied in the qualification "virtually perfect;" such a phrase

[8] n = 6 to 28; cf. Culler, *op. cit.*, 310.
[9] Lufkin, *op. cit.*, 666.
[10] Culler, *op. cit.*, 310.

would naturally characterize the variable correlations found in experience and not the fixed relations of theory. It would be curious to say, for example, that the dependence of a circle's area on its radius is "virtually perfect." In a mathematical universe, the dependence of function on argument is in general unique and complete; in a perceptual universe, no such dependencies exist. The phrase "virtually perfect" was then meant to imply that we are dealing with a natural correspondence which rises and falls from one group of data to another as do all experimental relations; but which, in Urban's data, proved to be almost unity.

Urban's major (though not sole) mistake lay in applying to quantities which may correlate in any amount from zero to unity a formula (for probable error of a quotient) which applies only when numerator and denominator are *not significantly correlated*. The reason is clear. Given $a = c/h$: when c and h rise and fall together, a will be well-nigh constant; when they vary independently (as theory for propagation of error assumes), a will fluctuate far more. The higher this covariance, the larger the error thereby introduced; because of the extremely high agreement of c and h in Urban's data, his p.e. work out from 10 to 30 times their true value. When r is small the error is less, but will always remain in some degree so long as c and h correlate at all. When they vary inversely (correlate negatively), the Urban-values are too small.[11]

Far from believing the $\Phi(\gamma)$ procedure invalid, I have adopted Müller's weighting-device in deriving a set of tables (soon to be issued) applying to biological data the chemical equation for monomolecular autocatalysis.

University of Illinois. ELMER CULLER

A NEW SYSTEM FOR THE CLASSIFICATION OF ODORS

E. C. Crocker and L. F. Henderson of Arthur D. Little, Inc., of Cambridge, Mass., have recently published some very interesting results of research on the analysis and classification of odors.[1] This problem has persistently continued to fall short of a final, acceptable solution. Henning's prism may be thought of as reducing Zwaardemaker's nine classes to six as well as making a statement of the relationships between the six. Experimental attempts to verify Henning seem always to result in but partial verification: the prism is neither wholly right nor wholly wrong.[2] The psychologist may welcome, therefore, a further simplification, if not in the belief that any simple statement can give the complete account of olfactory quality, at least in the hope that the eventual establishment of some gross fundamental classification may furnish a safe ground for further experimental differentiation.

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[11]The former note, written under great pressure for time, contains a few places which might have been phrased more suitably or clearly. Two typographical errors may be listed: change 'without' (p. 308, last line) to 'within' and 'thought' (p. 311, c, line 7) to 'though'.

In this connexion let me add that Dr. S. M. Newhall kindly informed me of an error in my Studies in psychometric theory (xi), *J. Exper. Psychol.*, 9, 1926, 180, Table IV: after P replace 2.30 by 2.32, 5.98 by 5.59, .800 by .758. These changes, happily, reënforce my original conclusions.

Let me signalize again Professor Thomson's article, above cited, on this problem; it will repay—and demand—careful study.

[1]E. C. Crocker and L. F. Henderson, Analysis and classification of odors, *Amer. Perfumer and Essential Oil Rev.*, 22, 1927, 325 ff. The relative unavailability of this publication to academic psychologists is my excuse for the first part of this note, but I can not of course reprint most of the data nor summarize the paper completely. Mr. Crocker has furnished me with a number of reprints, and I shall be glad to send these to interested psychologists as long as the supply lasts.

[2]Cf. F. L. Dimmick, this JOURNAL, 33, 1922, 423-425; M. K. Macdonald, *ibid.*, 33, 1922, 535-553; A. E. Findley, *ibid.*, 35, 1924, 436-445; Dimmick, *Psychol. Rev.*, 34, 1927, 321-335.

Crocker and Henderson are chemists engaged in research in connection with the manufacture of perfumery. They have both had wide experience with smells, and each had worked at the classification of odors before their collaboration. As a result of a great deal of systematic observational work they have concluded that for purposes of classification there are four principal qualities of odor and that any particular odor may be placed with respect to others by a statement of the numerical degree by which each of these 'components' enters into it. The four basal qualities are:

(*1*) *Fragrant*, which corresponds to Zwaardemaker's and Henning's Fragrant.

(*2*) *Acid* (or sour), a new class. The authors reject Henning's Fruity (and also Zwaardemaker's Ethereal) on the ground that a quality thus described depends in part upon the tactual effect of vapor in the nostrils. "Acid" is thought by them to be the true olfactory element that remains when the effects of vapor are abstracted from. They also warn against the confusion with smell of the experience of "pungency," which they find frequently associated with acid smells and which they disregard as sensory pain. That they have not fallen into the "stimulus error" is apparent when they note that it is not acids alone that have the acid smell.

(*3*) *Burnt*, which is Henning's Burnt and Zwaardemaker's Empyreumatic.

(*4*) *Caprylic* (or oenanthic), which is Zwaardemaker's Hircine.[3]

Crocker and Henderson have established by practice an absolute scale of judgment for each of these four 'components.' The scale runs from 0 to 8, that is to say, it is a scale of nine points. Each odor is characterized numerically with respect to each of the four *Hauptgerüche*, which they write conventionally in the order FABC. Thus, when they say that cineol is 5726, they mean that cineol is placed at 5 in the scale for Fragrant (F), at 7 for Acid (A), at 2 for Burnt (B), and at 6 for Caprylic (C).

In this way they have worked out the class numbers of a great many smells. In their Table I they give 'standards' for each principle class, *i.e.* for eight substances representing the degrees of F from 1 to 8, combined with varying other degrees of A, B, and C; and similarly for the other three principal 'components.'[4] Classifications of other odors are made by re-

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[3]One can not safely speak of relationships between the classes used by different investigators because of similarity or difference of name merely; the ultimate reference must be to actual stimuli. Nevertheless the experimental indications of the following relationships are pretty clear.

Zwaardemaker	*Henning*	*Crocker and Henderson*
1. Fragrant	1. Fragrant	1. Fragrant
2. Ethereal	2. Ethereal	2. Acid
3. Aromatic	3. Spicy	
	4. Resinous	
4. Ambrosiac		
5. Alliaceous		
6. Empyreumatic	5. Burnt	3. Burnt
7. Hircine		4. Caprylic
8. Foul	6. Putrid	
9. Nauseous		

[4]Since all this discussion has scientific meaning only in relation to the stimuli, I reprint here the table of standards, omitting only a few alternative standards. I regret that I do not have space for reprinting Crocker and Henderson's Table II.

Fragrant		Acid	
1517	Cyclohexanone	6123	Beta-naphthyl ethyl ether
2424	Toluene	4221	Cedrene
3336	Alpha-chlornaphthalene	5336	Resorcinol dimethyl ether
4344	Alpha-naphthyl methyl ether	2424	Toluene
5222	Phenylethyl benzoate	5523	Isobutyl phenylacetate
6434	Diphenyl ether	5636	Methyl phenylacetate
7343	Safrol	5726	Cineol
8445	Benzyl acetate	3803	Acetic acid (20%)

(*Footnote 4 continued on page 344*)

ferring to these standards. In Table II the authors give the formulae for 75 chemicals arranged according to chemical nature. In Table III they give ranges for certain types of odors; for example, a typical perfume is F 6-8, A 2-6, B 2-4, and C 2-4; greasy odors are F 3-5, A 6-7, B 5-7, C 7-8; and so on for nineteen other types.

They also give some curves for the variation of F, A, B, and C in the alcohols and aldehydes where the number of carbon atoms changes, but they realize that they are as far from a chemical theory of smell as other investigators. They even suggest that the ultimate theory may turn out to be physical rather than chemical, depending upon surface tensions, etc.

The value of this work as a ground for psychological understanding of smells and further research upon them seems to me to depend upon its reliability. The authors have no elaborate theory; nevertheless, if it were possible for anyone, with sufficient practice in the use of these scales and in abstraction from non-olfactory components, to identify without error an odor by a four-digit number, then we should be in the way of defining stimuli for research, and we should have a system in terms of which research on mixture, reciprocal adaptation, and other problems could be intelligently undertaken.

Reliability. Crocker and Henderson gave no data upon reliability, but at my suggestion they have set to work and have supplied me with data showing the judgments of 25 stimuli made independently by Crocker (*C*), by Henderson (*H*), and by each of two untrained observers (*A* and *B*). Each *O* was given these 25 stimuli and, for comparison, the 32 standards. The untrained *O*s worked through the samples one at a time, without rechecking. *C* and *H* arranged the samples in order for each 'component' separately, comparing constantly with the standards, and then comparing similar numbers together and with the standards. It required from 90 to 120 min. for each *O* to complete this task.

The problem thus becomes one of correlations among *O*s, but in this case coefficients of correlations are less interesting than the average deviation of one *O* from another. Accordingly I have determined for each of the four principal olfactory factors and for each pair of *O*s the differences. Since there are four *O*s, there are six pairs of *O*s; 6 pairs × 4 olfactory judgments × 25 stimuli = 600 differences. The following summary shows the frequency of large and small deviations of one *O* from another. The most usual deviation was 1; there was agreement oftener than a deviation of 2; large deviations were rare.

Deviation	6	5	4	3	2	1	0	Av. = 1.22
Frequency	3	5	11	42	130	256	150	Tot. = 600

These data are analyzed further in the accompanying table, which shows average deviations for each pair of *O*s and for each class of judgment.

The table indicates in the first place that practice had less effect than would have been expected. *C-H* is the pair of experts (the authors), and experts might have been expected to deviate from each other less than untrained *O*s, and less than an expert deviates from a novice. It is true that average deviation of *C-H* (1.03) is the least of all, but the low value is largely determined by the small deviation for Caprylic Judgments

Burnt		Caprylic	
5414	Normal propyl alcohol	5221	Santolol
6322	Phenyl propyl alcohol	5322	Isoamyl benzoate
5336	Resorcinol dimethyl ether	5523	Isobutyl phenylacetate
4344	Alpha-naphthyl methyl ether	4314	Cyclo-hexane
5345	Veratrol	4445	Tetralin
6665	Thujone	5636	Methyl phenylacetate
4376	Para-cresyl acetate	2577	Anisol
7584	Guaiacol	3518	Diisoamyl

of C were peculiarly difficult to make, and here practice may have had its effect. The *C-H* pair does not give the minimal deviation for A and B, and for F the deviation with *C-H* is no less than with *A-C*. The psychologist starting out to duplicate these results ought not, then, to expect that a long period of training would be necessary.

It is, of course, only the psychologist, long baffled by the olfactory problem, who is surprised by this conclusion. Crocker and Henderson tell me that their experience shows that a new operator can begin immediately with fairly accurate evaluations. once he understands the instructions, and that familiarity with the system makes the judgments easier but not, in general, more accurate. Accuracy is, however, increased for both new and practiced operators by abstraction from three components and attention to only one at a time.

	Observer-pairs						
	A-B	*C-H*	*A-C*	*A-H*	*B-C*	*B-H*	Av.
Fragrant	0.92	0.76	0.76	0.88	1.12	1.16	0.93
Acid	1.12	1.32	1.24	1.44	1.72	1.48	1.39
Burnt	1.12	1.20	1.04	1.28	1.28	1.12	1.18
Caprylic	1.36	0.84	1.24	1.28	1.76	1.80	1.38
Average	1.13	1.03	1.07	1.22	1.47	1.39	1.22

The averages for the four 'components' indicate that F is by far the most accurately judged. This result is not surprising, for there has always been most to say and least disagreement about the fragrant odors. The greatest difficulty was experienced with the completely novel class, A, and the relatively novel class, C. B is intermediate.

Crocker and Henderson point out that their system provides the possibility of differentiating "9×9×9×9=6561 detectably different odors." However, the psychologist knows that accurate discrimination of nine degrees under these conditions is likely to be difficult to achieve; in fact it surprises me that the average deviation should be as small as 1.22. Nevertheless it is large enough materially to reduce the total number of accurate classifications with this method.

I have tried to get some impression as to the total number of accurate discriminations possible in the following way. The average deviation for F is 0.93 on a 9-point scale, 8 units long. Suppose that we increase the unit so that the points on the scale are separated by a difference equal to the probable error of the difference between them. In such a case we should just as often get perfect agreement as not, a way of dividing the scale into liminal steps. We can change 0.93 into a P. E. by multiplying by 0.845 (assuming normality of distribution); and, assuming that the same P. E. applies to two adjacent points, we can take the square root of twice its square for the P. E. of the difference and thus the new unit, which is for F 1.11. Similarly the unit for A is 1.66; for B, 1.41; and for C, 1.65. Now how many significant points can we get? The numbers of units in an 8-unit scale are as follows: F, 7.2; A, 4.8; B, 5.7; C, 4.8. Remembering that in a limited scale there is always one more point than there are units, we may say (roughly) that we might safely attempt 8 discriminations for F, 6 for A, 7 for B, and 6 for C. This result gives us 8×6×7×6=2016 supraliminally different odors.

If, instead of taking the averages of all pairs, we take the averages for the expert *C-H* pair, we come out by the same method with 3240 odors. The minimal average deviations, irrespective of the pairs that gave them, give 4410 odors. In general it seems that 2000, or possibly even 4000 odors can be distinguished from each other the majority of the time by this method and in terms of this system.

This result seems to me to be extremely promising. There is relatively great consistency for this difficult task. It is certainly worth the psychologist's while to see if he can do as well or better in the psychological laboratory, and, if he can, to repeat the attack upon some of the old problems of smell with this system behind him.

Harvard University EDWIN G. BORING

A NOTE ON THE INTERCORRELATION OF INTELLIGENCE TESTS

The following data were derived from the testing of eighty-eight children, nine years of age, with Series II, Examination C, of the Dearborn Group Test of Intelligence. This examination is made up of four parts: (1) picture sequence; (2) word sequence; (3) form completion; and (4) sentence completion.

The correlations here presented will be of interest to those who are concerned with the intercorrelations of the parts within a test, and the correlations of the parts with the total score. It will be seen, of course, that this study bears directly on the issues raised by the Army test results[1] regarding the value of tests having high or low intercorrelations of parts and high or low correlation of parts with the total scores.

Now, inasmuch as the correlations between scores of parts with the total scores are in a degree self-correlations, it is necessary to equalize the possible influence of each on the total score. If each section contained the same number of items and had the same time allotment, no weighting would be necessary. But inasmuch as time and possible maximum score vary for the several parts, scores were weighted; and for these the following coefficients of correlation were found.

Parts	*r*
I with II	.35 ± .06
I with III	.35 ± .06
I with IV	.42 ± .05
II with III	.11 ± .07
II with IV	.50 ± .05
III with IV	.23 ± .07
Total with I	.69 ± .03
Total with II	.69 ± .03
Total with III	.57 ± .04
Total with IV	.77 ± .03

If these results are valid and typical, it does not seem at all necessary to hold with some investigators that in constructing tests we must choose between the mathematical desideratum of low intercorrelations but high coefficients with the totals (or criterion), on the one hand, and the so-called "psychological fact," on the other. This "psychological fact" has been phrased in the question, "Are there tests, as a matter of fact, which have a low intercorrelation and which correlate to a high degree with a criterion; in other words, can tests be found to meet this mathematical desideratum?"[2] From the foregoing coefficients, it appears that there is fairly good reason to answer this question in the affirmative.

In the six intercorrelations there is only one which is not significantly below the coefficients obtained for any of the parts with the whole. That is the coefficient of .50 for II and IV (word sequence and sentence completion). The reason for this intercorrelation, somewhat higher than the rest, lies in the fact that both II and IV involve language; yet .50 is sufficiently far removed from unity to enable one to say that these parts are not necessarily measures of the same fact. However, it will be noted that

[1] Psychological Examining in the U. S. Army, *Memoirs National Academy*, 15, 1921, 316, 338.

[2] F. N. Freeman, *Mental Tests*, 1926, 238.

this correlation of .50 is well below those for parts II and IV with the total, .69 and .77 respectively.

It must be remembered that the question of "psychological fact," quoted above, was raised in connection with the results obtained with Army tests. No one will deny that these tests were of great service in the solution of a pressing problem. But at the same time it should also be clear that the results were obtained under conditions which by no means warrant their acceptance as data out of which to build principles. If the problem of the intercorrelation of parts and correlation of parts with total scores or criteria is to be dealt with, data must be obtained under reliably controlled conditions, such as were impossible in the Army.

Cornell University FRANK S. FREEMAN

BLUFFING IN EXAMINATIONS

Fernberger's recent note[1] on the prevalence of bluffing in examinations suggests that the matter be investigated by means of objective type questions. The following three questions were accordingly inserted in the first hour-quiz given students in elementary psychology at Wesleyan University and at Antioch College. The topic covered in the preceding instruction period had been the measurement of individual differences. "Do not guess" instructions were given and it was implied that there was a true answer to each item. The quiz was taken by 46 students at Wesleyan and 84 at Antioch.

"(51) Psychoterminality is: (a) discussion of endless possibilities; (b) a neologism; (c) prophesy of maximum mental development; (d) result of feeding thyroid extract.

"(54) The trait of sexagenarianism is measured by: (a) one test in Army Alpha; (b) tests of sex differences; (c) a special test; (d) no kind of psychological test.

"(56) Tests of nevrility: (a) correlate highly with emotional balance; (b) have low correlation with intelligence; (c) are very reliable; (d) have not yet been made."

The following table summarizes the results. The three headings might be interpreted to read: knowledge, intelligent ignorance, bluffing.

Question	Group	Answers returned—in percentages		
		Right	Omissions	Wrong
51	Wesleyan	2	33	65
	Antioch	2.5	45	52
54	Wesleyan	37	43.5	20
	Antioch	36	52	12
56	Wesleyan	28	35	39
	Antioch	18	63	19
All	Wesleyan	22.3	37.2	41.3
	Antioch	18.8	53.3	27.6

These results do not cause one to rise in alarm; I am not sure that the showing is not better than I had thought possible. Call it 'luck' if you will, but only about a third fell into the trap digged for them.

But there is a moral. The students were at first inclined to be a bit indignant at being thus tricked. But the announcement that hereafter other such questions would be a matter of routine seemed to have a salutary effect in persuading them that it is necessary to know when you do not know.

Antioch College HORACE B. ENGLISH

[1] S. W. Fernberger, On bluffing in examinations, this JOURNAL, 38, 1927, 155-156.

Memory Color and Tuberculosis

During a period of three and a half years as a patient in a hospital for tuberculosis I found that the phenomenon of memory color[1] was quite frequently experienced by patients who had had a hemorrhage of the lungs. Tuberculous patients invariably examine every expectoration because they soon learn to determine whether their sputum is positive or negative. If it is greenish-yellow in color it is generally positive. Ordinary catarrhal droppings and sinus secretions are more yellowish.

For many months following hemorrhage of the lungs, tubercular patients have fleeting impressions when they expectorate white or yellow matter that it is red in color. Over 95% of all hemorrhage cases report this illusion. The larynx is slightly congested while expectorating. This gives one the impression that the mucous comes from the lower passages. Any congestion in the larynx is likely to arouse a 'beware of hemorrhage concept' and the illusion. In the majority of cases the 'red' vanishes in a few seconds, but in some it persists for two or three hours. Color discrimination is normal for the patients except under these peculiar circumstances.

University of Chicago Ray Mars Simpson

Ninth International Congress of Psychology

The Ninth International Congress of Psychology will convene at Yale University in New Haven, Connecticut, in the first week of September, 1929.

The officers of the Congress are as follows: President, J. McKeen Cattell; Vice-president, James R. Angell (Yale); Secretary, Edwin G. Boring (Harvard); Treasurer, R. S. Woodworth (Columbia); Foreign Secretary, Herbert S. Langfeld (Princeton); Executive Secretary, Walter S. Hunter (Clark); Chairman of the Program Committee, Raymond Dodge (Yale); Chairman of the Committee on Arrangements, R. P. Angier (Yale).

Besides these men the National Committee includes J. E. Anderson (Minnesota); Madison Bentley (Illinois); E. A. Bott (Toronto); H. A. Carr (Chicago); Knight Dunlap (Johns Hopkins); S. W. Fernberger (Pennsylvania); William McDougall (Duke); W. B. Pillsbury (Michigan); C. E. Seashore (Iowa); L. M. Terman (Stanford); E. L. Thorndike (Columbia); H. C. Warren (Princeton); M. F. Washburn (Vassar); R. M. Yerkes (Yale).

The first three officers and all but three members of the National Committee were elected by the American Psychological Association by mail ballot.

This is the first meeting of the Congress in America. The list of meetings with their presidents is as follows: Paris, 1889 (Charcot); London, 1892 (Sidgwick); Munich, 1896 (Stumpf and Lipps); Paris, 1900 (Ribot); Rome, 1905 (Sergi); Geneva, 1909 (Flournoy); Oxford, 1923 (Myers); Groningen, 1926 (Heymans); New Haven, 1929 (Cattell).

It is expected that the Congress in the United States will be truly international in character. The Americans hope that the appointment of some foreigners for lectures and lectureships can be arranged near the time of the Congress, so that foreign attendance can be increased and international solidarity within psychology furthered. Most appointments of this kind would have to apply to psychologists who speak English.

E. G. B.

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[1] Cf. G. K. Adams, An experimental study of memory color and related phenomena, this Journal, 34, 1923, 359-407.

The New York Meeting of the American Orthopsychiatric Association

The fifth annual meeting of the American Orthopsychiatric Association was held at the Russell Sage Foundation, New York City, on February 24th and 25th, with an attendance of over 200 psychologists, psychiatrists and psychiatric social workers. Four sessions were held for the presentation of scientific papers. On the evening of the 24th the annual dinner was held, followed by the address of the president, Dr. K. A. Menninger, on "What is orthopsychiatry?"

One session was devoted primarily to papers concerned with measurements. Dr. L. N. Yepson reported on "Objective estimation of social behavior." He used a graded series of statements about specific responses of the subjects, and had each rater check the item under every trait which best described the subject's behavior. Dr. P. H. Furfey described "A scale for measuring mental age," Dr. G. J. Rich reported on "Body acidity as related to emotional excitability," and a paper by Dr. C. W. Darrow on "Some physiological aspects of repression" presented some interesting differences between the effects of various stimuli as shown by the psychogalvanic reflex and the blood pressure. The leading paper of this session, "The study of the criteria and definitions of different groups of abnormal personalities," by Dr. William Healy, outlined four principal types of disordered personality which could be clinically differentiated.

The remaining sessions were devoted mainly to topics concerned with behavior disorders in children. Several of the papers were of especial interest to psychologists. Dr. H. M. Adler and Mr. S. H. Tulchin discussed the "Intelligence of criminals." Data from over 10,000 individuals yielded results in harmony with those which have recently been published by other investigators. Dr. C. Shaw considered "The correlation between the rate of male juvenile delinquency and certain indices of community organization and disorganization." Two papers, by Drs. S. T. Orton and P. Blanchard, were concerned with reading disabilities, more especially their effect upon the child's behavior as a whole. Dr. J. Levy reported tabulations of the size of the family of children who are referred to a clinic as behavior problems and showed that, contrary to general belief, the only child is not especially liable to become a problem. Dr. S. T. Orgel reported "A genetic study of mongolism," in which he was unable to isolate any causative factors for the condition, but was able to show that many of the factors which have been postulated as causes cannot be effective.

The meeting is significant in that it forms one of the few common grounds where psychologists, psychiatrists and workers in the social sciences can come together to discuss problems which require for their solution the coöperative efforts of these several points of view and techniques. The need of such a meeting is shown by its rapid growth in a very few years.

Institute for Juvenile Research Gilbert J. Rich
Chicago, Illinois

The Book Review Department

Professor Joseph Peterson of the George Peabody College for Teachers, of Nashville, Tennessee, assumes, with this number of the Journal, the editorship of the department of Book Reviews. This department will be greatly strengthened; more space will be devoted to it than has been given it in the past, and emphasis will be placed upon constructive and critical reviews rather than upon mere abstracts.

K. M. D.